COMMENTS SUMMARY REPORT

EXTERNAL PEER REVIEW OF THE ROLE OF NITROGEN/PHOSPHORUS IN CAUSING OR CONTRIBUTING TO HYPOXIA IN THE NORTHERN GULF

Prepared for:

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I. INTRODUCTION

The United States Environmental Protection Agency (EPA), Office of Water is charged with protecting ecological integrity and human health from adverse anthropogenic, water-mediated effects, under the purview of the Clean Water Act and Safe Drinking Water Act. In support of this mission, the Health and Ecological Criteria Division develops health criteria, ecological criteria, and technical guidance documents for water and water-related media.

EPA scientists in Region 4 (Atlanta) conducted an internal examination of science and data on conditions controlling hypoxia in the northern Gulf of Mexico. These scientists produced a draft report for Region 4 management drawing specific conclusions regarding the role of phosphorus as the limiting nutrient controlling Gulf hypoxia. A revised draft was submitted for broader internal review in April 2004. This staff report was found to lack the science and data necessary to conclusively support the findings as presented. However, the report did raise a number of important scientific questions. After further refinement, the report was released as an informational document from EPA Region 4 to the Hypoxia Task Force to encourage discussion and pose questions for the reassessment of the Hypoxia Action Plan. The report, "Evaluation of the Role of Nitrogen and Phosphorus in Causing or Contributing to Hypoxia in the Northern Gulf, August, 2004," was released without external peer review.

The August report, as well as the earlier drafts that were not prepared for release but have been circulated outside EPA, raised concerns within the Task Force and the Stakeholders throughout the Mississippi Basin. As a result, EPA Region 4 requested and the Task Force agreed at its August 31, 2004 Executive Session that the Monitoring, Modeling and Research Workgroup (MMR) co-chairs coordinate a rapid scientific peer review of the August 2004 draft Report. After concerns were raised at the September 1, 2004 public meeting of the Task Force that earlier versions of the report should also be included in the review, the Coordinating committee, in coordination with the MMR, examined several options and concluded that adapting the process employed by EPA to seek expert reviews of internal scientific and policy papers would allow for appropriate input from the Task Force while maintaining an independent and impartial review.

Peer review is an important component of the scientific process. It provides a focused, objective evaluation of the document or materials submitted for review. The criticism, suggestions and new ideas provided by the peer reviewers stimulate creative thought, strengthens the reviewed document and confers credibility on the product. Comprehensive peer reviews lead to good science and product acceptance within the scientific community. Under this work assignment Dr. James Ammerman, Dr. Donald Boesch, Dr. Walter Dodds, Dr. Robert Howarth, Dr. Steven Lohrenz, Dr. Gregory McIssac, Dr. David Millie, Dr. Hans Paerl and Dr. Andrew Sharpley.

II. CHARGE TO THE PEER REVIEWERS

1. Do the data presented provide sufficient evidence for P limitation of phytoplankton growth?

2. Do the data presented provide sufficient evidence for N limitation of phytoplankton growth?

3. Do the data presented support the conclusion that point source P contribute to algal blooms and gulf hypoxia?

4. Is the observed trend on P loading consistent with Gulf reactive P increase?

5. Are there other mechanisms that cause the N and P Increase in the Gulf?

6. Do the presented data support N and P reduction goals on a seasonal cycle?

7. Significance of data from USGS monitoring stations below St. Francisville?

8. The Redfield Ratio as used by Rabalais et al. (1999) in the Mississippi River has been called into question? Please comment on the use of the Redfield Ratio in the Region 4 Report Evaluation, and the use of this calculation to identify the limiting nutrient in the water column of the Mississippi River and the Northern Gulf of Mexico.

9. Please identify gaps in the data and information, and provide additional references and data resources where possible.

III. GENERAL COMMENTS

Reviewer 1

I wholeheartedly agree with the concept of a balanced approach to managing both nitrogen (N) and phosphorus (P) at their source and during transport to decrease loads, which are “significantly” above historic background levels in or to restore water quality throughout the Mississippi and Alchafalaya River Basins, as well as to decrease Gulf hypoxia. Thus, the premise of the recommendations being made in this document is sound. However, I was disappointed with the poor editing of the final August 2004 document and have made numerous notations directly on the manuscript. For such an important document, I was particularly concerned by the lack of consistent spelling of phosphorus (phosphorous) correctly throughout, as this was the main nutrient of concern or contention. Additionally, numerous references are cited in the text but not listed at the end. This is disappointing for such a high level document and for which has presumably been reviewed several times already. The authors should not use the confusing term “reducing” when referring to a lower or decrease in a value, especially when the text is discussing oxidation state of water and estuarine sediments. “Reduce” refers to an anoxic state and should be avoided (see pages 14 and 15).

On a more positive note, several gross assumptions have had to be made, which unfortunately could lead to major land management and nutrient use changes within the Basins. Even so, a holistic sustainable approach to nutrient management is essential to deal with the major concerns and issues. For too long, land used have addressed and managed only one of the nutrients through improved or conservation practices and strategies. This has lead to numerous situations where managing for one nutrient has compromised or increased the loss potential of the other.

Farm N inputs can usually be more easily balanced with plant uptake than can P inputs, particularly where confined livestock operations exist. In the past, separate strategies for either N or P have been developed and implemented at farm or watershed scales. Because of different critical sources, pathways, and sinks controlling P and N export from watersheds, remedial efforts directed to either P or N can negatively impact the other nutrient. For example, basing manure application on crop N requirements can increase soil P and enhance potential surface runoff losses. In contrast, reducing surface runoff losses of P via conservation tillage can enhance N leaching. These positive and negative impacts of conservation practice on resultant water quality should be considered in the development of sound remedial measures. See Table 1 for more detail. Clearly, a technically sound framework must be developed that recognizes critical sources of nutrient export from agricultural watersheds so that optimal strategies at farm and watersheds scales can be implemented to best manage both P and N and effect a decrease in nutrient loads in the Mississippi and Alchafalaya River Basins and to the Gulf.

The lag time between implementation of any conservation practice or remedial strategy and water quality improvement can often exceed the monitoring period due to limited long-term funding opportunities (National Research Council, 2000a). Despite our knowledge of controlling processes, it is difficult for the public to understand or accept this lack of response. When public funds are invested in remediation programs, rapid improvements in water quality are usually expected and often required. Thus, implementation of effective remedial strategies to decrease nutrient loads in the Mississippi and Alchafalaya River Basins, should consider the re-equilibration of watershed and lake behavior, where nutrient sinks may become sources of N and P with only slight changes in watershed management and hydrologic response. Education programs should also be established to highlight the long-term benefits of remedial measures.

The time of Mississippi and Alchafalaya River Basins and associated northern Gulf estuarine response to changes in nutrient management strategies and implementation of conservation practices is particularly important for P, due to its long residence time in ecosystems, compared to N. Studies have shown that even where P applications are stopped, elevated soil P can take up to 20 years to decline from crop uptake and removal to levels at which crops will respond to additional applications. Also, internal recycling of P in estuarine sediments can supply sufficient P to maintain eutrophic conditions in P-sensitive waters.

Table 1. Nutrient management measures to control nonpoint sources of agricultural N and P.

Practice	Description	Conservation practice code [†]	Impact on loss [¶] N	Impact on loss [¶] P
Farm-Gate				
Crop hybrids	Low phytic-acid corn reduces P in manure	592	neutral	decrease
Feed additives	Enzymes increase nutrient utilization by animals	592	decrease	decrease
Feed supplements	Match animals nutritional requirements	592	decrease	decrease
Livestock selection	Group animals according to nutrient requirements	592	decrease	decrease
Soil / Plant Assessment and Management				
Crop requirements	Nutrient applications based on crop N &/or P needs	590	decrease	decrease
Pre-sidedress Nitrogen Test	PSNT can aid accurate split N applications	590	decrease	neutral
Soil P testing	Nutrient applications based on soil P availability	590	neutral	decrease
Tissue testing	N applications can be tailored to crop needs	590	decrease	neutral
Cover crops/residues	If harvested can reduce residual soil nutrients	340	decrease	decrease TP increase DP
Crop rotation	Sequence different rooting depths to recover N & P	328	decrease	decrease
Conservation tillage	Reduced and no-till increases infiltration and reduces soil erosion	329	decrease TN increase NO_3	decrease TP increase DP
Strip cropping, contour tillage, terraces	Reduces transport of sediment-bound nutrients	330, 585, 600, 660	decrease TN neutral NO_3	decrease TP neutral DP
Conservation cover	Permanent vegetative cover increases soil infiltration and water holding capacity	327	decrease	decrease
Soil amendment	Flyash, Fe oxides, gypsum reduce P solubility	590	neutral	decrease
Invert stratified soils	Redistribution of surface P through profile by plowing	324	neutral	decrease
Site-specific management	Use of GIS & GPS to apply and manage nutrient sources	342, 462	decrease	decrease
Buffer, riparian, wetland areas, grassed waterways	Removes sediment-bound nutrients, enhances denitrification	332, 393, 391, 412, 601,607, 608, 656	decrease	decrease TP neutral DP
Critical source area treatment	Target sources of nutrients in a watershed for remediation	590	decrease	decrease
Application Decisions				
Method of application	Incorporated, banded, or injected in soil	370, 590	decrease	decrease

Rate of application	Match crop needs	633	decrease	decrease
Source application	Sources can differ in their P & N availability	590	decrease	decrease
Timing of application	Avoid application to frozen ground Apply during season with low runoff probability	370, 590	decrease	decrease
Amendment	Adding alum to manure reduces NH_3 loss and P solubility; nitrification inhibitors can slow leaching and gaseous loss of N	359, 370, 590	decrease	decrease
Physical treatment	Separation of solid and liquid manure phases and chemical additions	359, 370	decrease	decrease
Barnyard management	Reduce runoff, capture and treat manure slurry and rainfall runoff	370, 558, 570	decrease	decrease
Composting	Increases bulk density and uses for manure	317	decrease	decrease
Digestion	Aerobic and anaerobic manure digestion produces energy and reduces gas emissions	365, 366, 370	decrease	neutral
Manure storage	Lagoons, pond storage	313, 359, 370	decrease	decrease
Transfer	Move manure from area with surplus to deficit nutrients	634	decrease	decrease

† USDA-NRCS National Conservation Practices Standard Codes from **http://www.nrcs.usda.gov/technical/Standards/nhcp.html**

¶ TN is total N, NO_3 is nitrate, TP is total P, and DP is dissolved P.

Reviewer 2

Hypereutrophy and its associated detrimental water quality impacts are catastrophic and growing problems on local, regional, national and worldwide scales. Nutrient loading is a key factor responsible for accelerated eutrophication within aquatic systems and the Gulf of Mexico (GOMx) has not escaped this horrific anthropogenic impact. In particular, increased phytoplankton production, in response to increased nutrient loading has been identified as one (of several interacting) critical stimulus (stimuli) forcing the occurrence and extent of GOMx hypoxia.

The report, 'Review of Issues Related to Gulf of Mexico Hypoxia' produced by EPA scientists attempts to synthesize reports from multiple Task Forces, but results in a summary of what the EPA views as the key management recommendation (i.e. reduction in phosphorus loads rather than reduction in total dissolved inorganic nitrogen loading) to control hypoxia. The authors contend that "…there is no convincing data that suggest that phytoplankton growth occurs in late summer and fall….", when nitrogen is surmised to be the limiting nutrient for GOMx assemblages. While data (and prose) is presented to justify this viewpoint, I feel that the EPA's concluding recommendation to control one nutrient (while dismissing the potential overall impact of nitrogen) is incredibly nearsighted. A total pessimist might even consider such a viewpoint to be driven by an agency's political desire to maintain the status quo of agribusiness (which incorporates excessive nitrogen fertilization within row crop agriculture throughout the Midwest drainage basin), rather than a quest to maintain and/or improve ecosystem-level health and sustainability.

Identification of nutrients controlling microalgal growth is critical for predicting the occurrence of and/or controlling phytoplankton. Invariably, nitrogen and phosphorus are the nutrients of choice when considering input reductions potentially effective for bringing freshwater and marine systems into nutrient-limited, bloom-free conditions. Studies have indicated that offshore waters of the GOMx are nitrogen limited whereas near shore waters may be phosphorus limited. However, the (straight-forward?) cause and effect relationship between these variables and phytoplankton biomass and (or) growth rate (as it may or may not relate to Redfield requirements) is difficult to develop. Combination of nutrient-based loadings and resulting co-limitations often exist within dynamic productive systems and could conceivably (and alternatively) promote high growth rates of selected phytoplankton, especially when phytoplankton biomass is low and other factors are not limiting. For example, although dissolved inorganic nitrogen appears to be a determinant parameter for algal biomass, phosphorus availability would be immediately expected to directly affect production and (or) growth potential when nitrogen supply exceeds demand (i.e., during periods of river inflows).

The report stresses that 'on the basis of calculated Redfield ratios, dissolved inorganic nitrogen would have to be reduced greater than 75% to at best achieve 16:1 nitrogen:phosphorus ratio. It seems to be have been forgotten that contrasting nutrient compound reactivities translates into physiologically- and taxonomically-distinct phytoplankton responses. As such, the timing, modality, and composition of nutrient inputs/dynamics play key roles in determining species-specific phytoplankton growth responses and ultimately, community structure, and biodiversity in many hypereutrophic systems. Further, in the absence of nutrient limitation, light becomes the primary limiting factor for photosynthesis and growth in aquatic environments. Because nutrient metabolism and photosynthetic activity are coupled, those phototrophs most efficient at light capture under light-limiting conditions will become the competitive dominants and eventually monopolize the resource. From this, it can be concluded that linking N and P inputs/dynamics to overall community responses (while seemingly minimizing species-specific responses) may lead to generalized and erroneous conclusions.

As already stated, eutrophication of coastal waters has resulted in catastrophic impacts, including hypoxia. The Hypoxia Action Plan (resulting from integrated assessments of the initial Task Forces) recommended a 30% reduction in nitrogen loading within the drainage basin of the Mississippi River. I cannot understand why the EPA would not recommend reducing ALL NUTRIENT LOADING by at least this amount, if not significantly greater (rather than attempting to focus solely on minimizing phosphorus loads as the means to reduce phytoplankton growth). Moreover, little mention was made of replacing wetland and estuarine systems lost as chemical filters and processors of terrestrially-derived nutrients and pollutants. The loss of wetland habitats throughout the Mississippi/Atchafalaya River Basins has been horrific – in large part brought about by ‘channelizing’ these rivers for shipping and commercial trade. The authors contend that nitrogen loading has not increased (on average) throughout the last twenty years – they fail to mention the corresponding and appalling loss of filtering capacity associated with these wetlands. If one wants to attempt to manage the effects of nutrient enrichment arising from, as well as the existence and functionality of these systems, I would recommend dramatic action plans to restore the crucial wetland habitats and ‘dechannelize’ the current waterways. Such attempts would help curtail the direct ‘piping of nutrients’ from Midwest agriculture into the GOMx.

To the authors’ credit, additional data collection and research needs were indicated by the regional scientists. EPA scientists indicated that “…an adequate monitoring program in the GOMx would require significant additional financial resources and commitment of the various federal agencies…” I could not agree more – to obtain more knowledge concerning specific action items deemed critical for making informed management decisions, all federal agencies have to collaborate and ‘ante up’ research and management dollars.

Reviewer 3

a. **Poor Understanding of Marine Eutrophication**.

It is clear that the authors of the Region 4 report approached their analysis from a very traditional limnological perspective. It is quite surprising that they seem to be so unfamiliar with the marine eutrophication literature. Their failure to cite the landmark National Research Council (2000) report, *Clean Coastal Waters: Understanding and Reducing the Effects of Nutrient Pollution* (a study co-sponsored by the U.S. Environmental Protection Agency!), probably reflects this unfamiliarity and is unfortunate because a reading of this treatise would have saved them from numerous misconceptions. In particular, the evidence and rationale for excessive N loading as a primary cause of coastal eutrophication in contrast to greater importance of P loading in fresh waters have been reported for some time (e.g., Hecky and Kilham, 1988). This has been variously attributed to the less efficient co-precipitation of P, the aforementioned lower cyanobacterial fixation, and higher rates of denitrification in nearshore marine and estuarine systems. More recently, Blomqvist et al. (2004) attributed this difference simply to “the matter of salt.” Because of the higher concentrations of dissolved sulfate in salt waters, the iron oxyhydroxides that typically sequester P in freshwater systems are not able to in marine waters, where there is enhanced iron sequestration by sulfide in sediments. As a result, P is effectively recycled and returned to the water column (this is briefly acknowledged in Section 5.2.3 of the January and April drafts of the Region 4 report). Such recycling is greatly increased when bottom waters become anoxic and sulfate reduction dominates microbial processes. In that way hypoxia formation provides a strong positive feedback, supplying P needed to sustain the primary productivity that then sustains or exacerbates hypoxia.

b. Dynamic, Spatially Realistic Context of Hypoxia.

It never ceases to amaze how many critics of the CENR integrated assessment, and even many ocean scientists, lack a grasp of the geography, scale and physical dynamics of the Louisiana continental shelf and shelf hypoxia. They tend to focus heavily on the Mississippi River plume, that is, the immediate dilution plume emanating from Southwest Pass of the Birdsfoot Delta, a zone extending 100 km or less from this river mouth. This is, in part, because of the large number of studies that have been conducted in the plume, attracted by the intense gradients that allow one to study the uptake of nutrients and how they fuel organic production and the food chain. For the most part, however, this plume drifts atop a relatively deep water column, 50 to 100 m or more, while hypoxia is only recurrent in bottom waters on the inner shelf, generally between 5 and 30 m deep and extending hundreds of kilometers along the coast. The plume domain is exemplified by the extent of P-enriched surface waters shown in Figure 1. Inside this boundary, there are strong gradients of concentration of DIP as river water is diluted by mixing with Gulf water and reactive P is taken up by growing phytoplankton. Outside of the boundary, DIP levels during spring and early summer are, as the Region 4 report demonstrates, often extremely low and there are no obvious surface concentration gradients extending from the plume area.

The hypoxia-prone zone extends along an over 400 km band closer inshore. It is overlain by highly productive surface waters, as illustrated in the color-IR image in Figure 1, which is taken from the photograph appearing in the *Science* news article about the EPA Region 4 report controversy (Ferber, 2004). This high-chlorophyll band represents the Louisiana Coastal Current (not strictly a river plume but a generally westward flowing coastal boundary current), which extends from the plume, through the C-transect and frequently all the way into Texas.

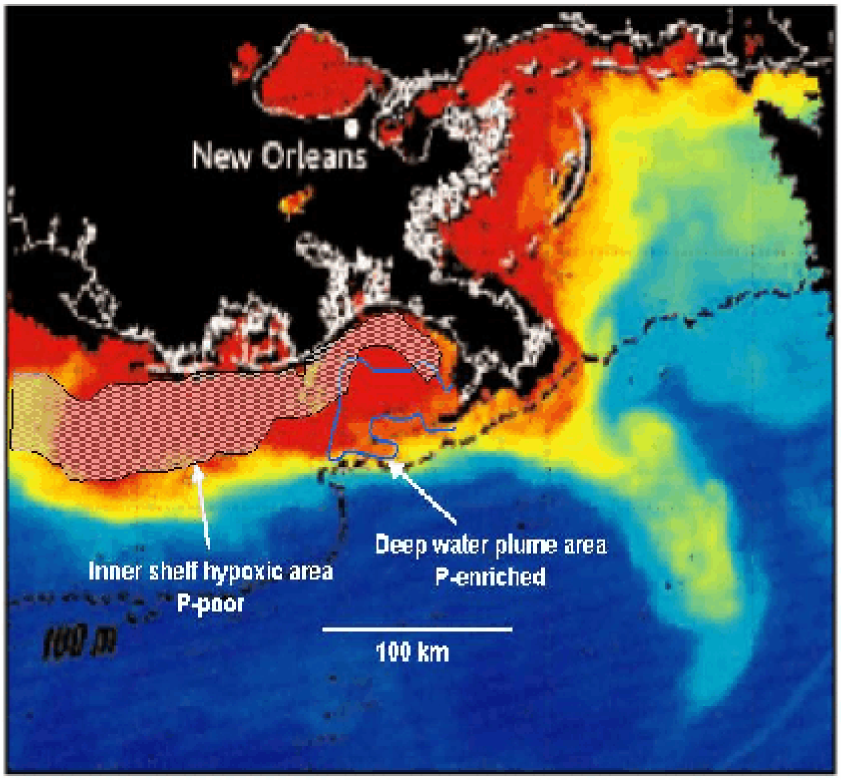


Figure 1. Relationship of the Mississippi River dilution plume with DIP concentrations > 0.03 µM in May 2001 (J. Ammerman et. al., unpublished), bottom water hypoxia (DO < 2 mg l^{-1}) in mid-summer 2001 (N. Rabalais, unpublished), and surface waters with high chlorophyll concentrations (undated image from Ferber, 2004).

In these surface waters it has been observed by Nelson (as quoted in the Region 4 report), Ammerman and colleagues, and Region 4 that in spring DIP concentrations may be vanishingly low, DIN:DIP ratios and alkaline phosphatase (AP) activity very high, and phytoplankton growth enhanced more by additions of P than N. By all accounts, phytoplankton production is severely P limited, yet enigmatically there is high phytoplankton biomass and productivity, days to weeks downcurrent from the relatively phosphorus-rich river plume. With such strong P limitation, how can this be? I offer the following interpretation stated as series of hypotheses:

i The primary production in the immediate dilution plume of the river contributes very little to the formation of hypoxia because most of it is deposited into a deep, thick water mass below the pycnocline, taking with it large quantities of nutrients.

ii. Organic production in the Louisiana Coastal Current (LCC) is primarily responsible for fueling oxygen depletion of underlying bottom waters on the inner shelf. This production is supported by N that escapes the dilution plume (the P supply runs out in the plume first) and recycled nutrients.

iii. Supplied with ample quantities of nitrogen, high primary production is sustained in the LCC by rapid and efficient recycling of P (high AP activity) in the surface layer and by reactive P brought up, often episodically, from below the pycnocline (where it is abundant). Because of high N availability, warm temperatures and ample light, the recycled P is taken up as fast as it is supplied, maintaining the dissolved P pool at very low levels even though significant biomass is being produced. The source of this P is from recently mineralized organic matter, sediment reservoirs and saline bottom waters advected onto the shelf from the Gulf of Mexico basin. [The majority of the P budget of Narragansett Bay and 30% of the P budget of Chesapeake Bay is supplied by inflows of shelf water, for example.] As hypoxia develops in the spring and early summer there is massive release of reactive P from bottom sediments (see Rabalais et al., 1999, Fig. 6.17), supporting the production that sustains hypoxia through the summer, eventually consuming the nitrogen supply (exemplified by nitrogen limitation in late summer and fall). Remember the N in the system is always being leaked off by denitrification, while P is only lost by burial or net advection off the shelf (which may not be great considering that the deep basin is also a source of P).

iv. Reducing the loadings of reactive P from the river in the spring will not alter the P supply in the LCC surface waters principally responsible for fueling hypoxia. The P supporting this production is mostly older and there are large sediment reservoirs of potentially reactive P. Rather, reducing the supply of P from the river will result in greater P limitation in the dilution plume, possibly allowing more N to escape into the LCC and hypoxia to expand. [As counter-intuitive as this may seem, extension of the symptoms of eutrophication over a larger area when P loading was reduced without corresponding N load reductions has been demonstrated or suggested for the Himmerfjarden (Elmgren and Larsson, 2001) and Lanholm Bay (Rosenberg et al., 1990) in Sweden, the Rhine River-Wadden Sea (van Raaphorst and de Jonge, 2004), Chesapeake Bay (Hagy et al., 2004), and the Neuse River estuary (Paerl et al., 2004).]

c. Implications for Action Plan and Reassessment.

Although the above interpretation is admittedly speculative, it is consistent with more of the facts and our experience with marine eutrophication than the simplistic extrapolations drawn from DIN:DIP ratios. From the serious deficiencies in the Region 4 report and this alternate explanation I draw the following implications for the Action Plan and Reassessment:

i. Stay the course on implementing the N reduction goal. A significant, highly professional effort went into the Integrated Assessment and the Action Plan drawn from it. The peer-reviewed literature to support these conclusions and goals is too substantial and highly regarded to have confidence shaken by a seriously deficient draft report such as the Region 4 report. Furthermore, I know of no *bona fide* expert on marine eutrophication who would suggest that hypoxia in the Gulf could be alleviated without significant reduction of N loading.

ii. While the inclusion of P reduction goals should be thoroughly evaluated in the Reassessment, it is premature to add P goals without much more careful analysis. Based on the examples provided in 10.b.iv there may actually be a credible risk in increasing the size or severity of hypoxia by reducing P loading without also reducing N loading.

iii. Assess the reported reductions in point source loadings of P in the LMR as an adaptive management experiment (as per CENR, 2000). If indeed these loads were reduced as reported by Knecht (2002) and Sutula et al.'s (2004) estimates concerning bioavailability are accurate, there may have been an abrupt 13% reduction in annual average loading or reactive P after 1993. When adjusted for interannual variations in river flow, how did this affect shelf hypoxia? On first appraisal, it would be hard to conclude that the area of hypoxia was reduced as a result (Figure 2); in fact, one might conclude that grew instead, as the 10.b.iv case studies suggest it could. [Note 1993 itself was the year of the great summertime flood and an unprecedented (to that point in time) extent of hypoxia].

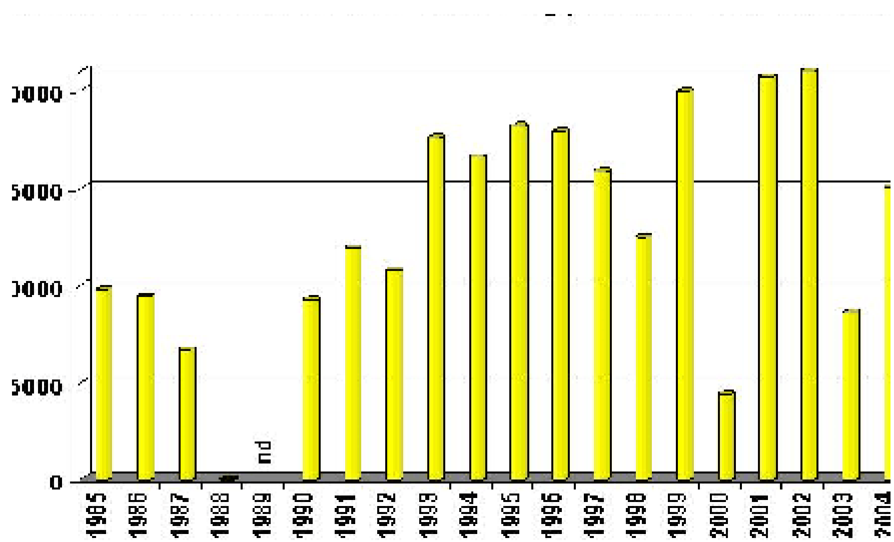


Figure 2. Estimated areal extent of hypoxia at the seabed of the northern Gulf of Mexico continental shelf as measured in mid-summer (LUMCON press release).

iv. Reject the advice in the January and April drafts of the Region 4 report that complex, 3-imensional hydrodynamic and water quality models must be created and calibrated before pollution reductions strategies are developed and implemented. Valuable time has been lost in restoration efforts in the Everglades and Chesapeake Bay while implementation was put on hold until we got the models right. Simpler, empirical models suggest that loadings of nitrate would have to be reduced from 30 to 45% in order to meet the Action Plan goal for attenuation of hypoxia (Scavia et al, 2004). At this point, it really doesn't matter where in that range the true target lies. Anything of that scope will be a heavy lift, so start taking actions to achieve significant N-load reductions now. Achieving these reductions will take many years, during which time the models and targets can be continually refined, in an adaptive management context.

v. Insist on open scientific debate and rigorous review in the Reassessment. I suspect that the Region 4 analysts got things so wrong because they were not involved in open, mainstream scientific dialogue. They would have had to be more familiar with the marine eutrophication literature if they had engaged in a dialogue with experts. Furthermore, the circulation of unreviewed, substandard work products did not serve EPA well and unnecessarily confused and set back the discussions about solutions.

Reviewer 4

Overall, I think the Region 4 report provides little new information or insight. The presence of phosphorus limitation on productivity in the northern Gulf of Mexico was recognized by Rabalais et al. (1999), and the problems with relying on elemental ratios alone to determine nutrient limitations were also discussed by Rabalais et al. (1999: pp 71-74).

The thrust of the Region 4 report appears to be criticism directed at an emphasis on N reductions in the Action Plan for the Gulf of Mexico. But it should be recognized that the Action Plan does not ignore phosphorus. It states: "While the primary focus of this strategy is on reducing nitrogen loads to the northern Gulf, many of the actions proposed through this plan will also achieve basinwide improvements in surface-water quality by reducing phosphorus as well." The suite of indicators to be monitored include reductions of P concentrations and loadings.

Within this context, the paper does not present any information on how much more attention should be directed at P reductions in order to achieve the "balanced approach" that they would like, nor does it outline how such a balanced approach might be determined.

Reviewer 5

Overview

The EPA reports of 2004, specifically the August 2004 Report, titled "Evaluation of the Role of Nitrogen and Phosphorus in Causing or Contributing to Hypoxia in the Northern Gulf", raise some fundamentally-important and pivotal questions, points and issues. These issues need to be addressed in order to clarify and definitively determine the mechanistic linkages between nutrient supply, productivity and excessive oxygen consumption (hypoxia) in the Northern Gulf of Mexico region draining the Mississippi/Atchafalaya Rivers. In particular, the reports propose that, based on molar nutrient concentration ratios in Mississippi/Atchafalaya river discharge to this region, phosphorus input reductions should be considered in addition to previously-agreed upon nitrogen reductions. It is argued that dual nutrient (N and P) nutrient reductions will most effectively and expeditiously control and reduce water column hypoxia in the receiving waters of the Northern Gulf of Mexico. Below are key questions and points the report raises, an evaluation of their validity, usefulness and applicability, and recommendations for pursuing answers to them.

Which nutrients (N or P, N and P) control new primary production?

Primary production provides the "fuel" for dissolved oxygen consumption ultimately driving hypoxia formation in stratified N. Gulf of Mexico waters under the influence of the Mississippi/Atchafalaya River Discharge Area (MARDA). The report justifiably questions some of the earlier conclusions by researchers and managers that nitrogen (N) is the key and (according to most recommendations) only nutrient source that controls new primary production fueling hypoxia. Based on a recalculation of the stoichiometric ratios of nutrient concentrations in the MARDA, the report correctly points out that questions should be raised about justifying N as the only limiting nutrient of concern, especially in fresh and brackish regions of the MARDA. Specifically, the report stresses that during the spring maximum productivity period, N:P ratios greatly exceed the 16:1 "Redfield ratio", considered by most biological oceanographers and estuarine ecologists to be the nutrient ratio required to achieve "balanced growth" and a useful index of potential nutrient limitation (i.e. < 16 indicates N limitation, > 16 indicates P limitation). Based on comprehensive data sets of nutrient concentrations at various locations near the mouth of the Mississippi River, and in the MARDA, it is shown that N:P elemental ratios in excess of 50:1 are common during the spring period of maximum production. Not until summertime (when primary productivity is relatively low) are ratios of 16:1 (but not lower) approached, indicating the potential for N limitation during that period.

The report correctly points out that some previous assessments of nutrient limitation based on stoichiometric ratios were improperly presented and when considered differently, would create a stronger case for potential P limitation (or N and P co-limitation). For example, previous nutrient concentration and input ratios published by Turner and Rabalais 1991, Justic et al. 1995, and Rabalais et al. 1999 were shown as DIN:Total P, as opposed to the more commonly used "conventional" ratio of DIN:DIP (dissolved N compounds over dissolved P compounds) (P. 1 of report). When it comes to biological reactivity, it makes more sense to consider the ratio of dissolved forms of one nutrient to another (dissolved species tend to be most readily available forms for uptake and growth) as opposed to comparing dissolved forms of N to *total* (dissolved *and* particulate) forms of P. The latter represents an "apple vs. oranges" approach to stoichiometric considerations of potential nutrient limitation and should probably not have been used. When one reconsiders the stoichiometric ratios as DIN:DIP, the absolute values for these ratios increase significantly, further arguing for potential P limitation in the MARDA. When considering these changes, the report correctly points out that a stronger case can and should be made for periodic P limitation in the MARDA (especially during springtime). Earlier "evidence" for nutrient limitation based on either stoichiometric ratios of nutrient concentrations and loads is probably biased towards N limitation (Pages 14-17).

While a case is made for periodic P limitation in the MARDA, the extrapolation of this case to a more general conclusion that point source P contributes significantly to the algal blooms in the Gulf of Mexico is not justifiable based on the stoichiometric arguments presented alone. Additional monitoring data (if available) and explicit experimental (bioassay- based) work are needed to support and confirm this statement. Limited bioassay-based results support this conclusion (Lohrenz et al. 1999, Dortch et al. submitted).

Excessive N loading: Its relationship to nutrient limitation and hypoxia dynamics

There is broad agreement that the Gulf of Mexico region under the influence of the Mississippi plume has, over at least the past 50 years, received increased anthropogenic N and P loads; to the point where this region is now considered "over-enriched" with both nutrients (P. 11 of Report). These loads have impacted (enhanced) primary production. To what extent nutrient (specifically N) loading has increased, such that in some areas of MARDA N uptake/utilization may be periodically saturated (Fig. 17, Jan. 2004 report) and some other factor (P, light, or both) is limiting remains unclear. There has been a great deal of discussion in all the versions of the report about "excessive N loading"; c.f. narrative of Fig. 17, "This figure suggests that DIN and DIP are being transported to the Gulf far in excess of that required to sustain the growth of phytoplankton". While the nutrient concentration and ratio (N:P) data provide evidence to support this argument, no evidence is presented to indicate that uptake of N compounds is in fact saturated (including relative to P uptake). If N saturation is indeed taking place in a substantial portion of the MARDA, either due to excessive supply rates and/or P limitation, then restricting P inputs to further limit primary production in this region might lead to an increase in N loading to more downstream N-limited marine waters, some of which overly the hypoxic zone. What would this mean for marine production, oxygen consumption and resultant hypoxia dynamics? This potential scenario is needs to be addressed and evaluated. Conversely, if only N (and no P) reductions are undertaken, can primary production in the MARDA be effectively reduced? The EPA document argues that N reductions would have to be much greater than 30% (at least 70% is mentioned on P. 26 of the Jan. 2004 report) to achieve this and that parallel P reductions will be most effective in reducing production and hypoxia potentials in the short run (see also P. 38 of the Jan. 2004 report and Abstract of April 2004 report). Nutrient concentration ratios are an *indirect* line of evidence to support the excessive N loading argument. In addition, *direct* lines of evidence, based on nutrient addition bioassays and kinetic uptake experiments are needed in order to let the “algae tell us” what nutrients are limiting and/or saturating in terms of supply vs. utilization rates. These bioassays should be conducted under *in situ* light and temperature conditions, using the naturally-occurring phytoplankton community as test organisms. In this regard, the “standard EPA bottle tests”, which are mentioned as a possible approach for examining nutrient limitations (P. 33 of Jan. 2004 report and Abstract of April 2004 report) are inappropriate and unrealistic for resolving nutrient limitation and biogeochemical ramification questions/issues given the strong interactions between light, temperature and seasonality (as well as temporal phytoplankton community compositional changes) known to take place in these waters.

The role of “other carbon sources” as drivers of hypoxia

There was a short discussion (section 5.2.4 in the April 2004 version of the Report) about “Impacts of Carbon on the Gulf Sediment Oxygen Demand”, that refers to researchers at the Univ. of Alabama (Carey et al. 1998, not cited in the report) concluding that “carbon inputs are a potentially significant contributor to oxygen demand and therefore the hypoxia of the inner shelf”. It is not clear which carbon sources and pools the report is referring to. Presumably, the authors are referring to *organic* carbon or watershed origin or organic carbon formed in the Mississippi River by primary production, but neither are specified. Furthermore, are the authors referring to *dissolved* or *particulate* sources of organic carbon? What is the overall evidence to support this and what are the relative contributions of externally-supplied (allochthonous) to internally-supplied (autochthonous, or “new” production) organic C sources? It is stated that “This carbon component should be included in the water quality model”, but without knowing what is specified and what quantities are being considered, it is difficult to support this conclusion. Stable C isotope measurements along the riverine-MARDA continuum may shed some light on this important issue. Some of these measurements may have already been made in conjunction with the N-GOMEX 2000 PROJECT: “An Integrated Monitoring and Modeling Assessment of the Oxygen Sources and Sinks in the Gulf's Hypoxic Zone, NOAA/Coastal Ocean Program, 2000 – 2004”, and NUMAN: “Utilizing Mississippi River Diversions for Nutrient Management in a Louisiana Coastal Watershed. USDA, 2002 – 2005” (see works of B. Wissel and B. Fry, Coastal Ecology Research Institute, LSU).

Other mechanisms/activities affecting N and P concentrations and loadings

It is possible and likely that changes in hydrology, land use, alterations of wetlands and changes in agricultural practices have affected and continue to affect nutrient loads to the MARDA and N. GOM. While this is a valid point and may be of importance in considering nutrient sources and their ultimate management with respect to productivity and oxygen dynamics, the report does not identify specific studies or data sources to evaluate and/or support the statement.

The roles of internal nutrient recycling in supporting productivity and hypoxia

It is generally agreed that in the shallow, periodically mixed waters characterizing the MARDA and receiving waters of the N. Gulf of Mexico, sediment-water column exchange and recycling are integral and important component of nutrient-production interactions ultimately implicated in and driving hypoxia

(P. 13-14 of the report). It has been argued that P recycling (in the water column and mediated by sediment-water column exchange) of historic and current P loads (either from anthropogenic or marine sources) may be sufficient to support "new" production for many years even if riverine P loads are immediately reduced. Conversely, at least some fraction of allochthonous N loads deposited in these sediments is likely to be "vented" from the system as relatively harmless N_2 gas via denitrification (Billen and Lancelot 1988, Seitzinger and Giblin 1996). P does not have a gaseous phase, hence the potential for differential N and P losses from the system exists. The relative recycling rates and subsequent availabilities of P and N need to be investigated and quantified. These rates have a great deal relevance to the development and implementation of long-term N and P input reduction strategies for the MARDA-GOM continuum. Furthermore, as N (or P) inputs are reduced and productivity is altered, the relative amounts and importance of internal cycling of these nutrients are likely to change as well. There is no assurance that these cascading events will occur in a linear or (at this point in time) predictable fashion. The various versions of the report refer to this important aspect of MARDA and N. GOM nutrient dynamics. It is premature to make recommendations pertaining to amounts and types of nutrient reductions needed to control production and hypoxia, given the lack of information on the relative roles and importance of internal N and P cycling. This is clearly an area of research and modeling need.

Role of N_2 Fixation

Nitrogen fixation can play an important role in supporting the N needs of some freshwater and marine ecosystems (c.f. Paerl 1990). In this regard, it *may* play a role in the Northern Gulf of Mexico waters (outside of the immediate influence of MARDA), which are known to be N limited and which periodically exhibit blooms of the filamentous N_2 fixing cyanobacteria *Trichodesmium* spp. Despite the presence of these blooms, it appears that these waters experience chronic N limitation throughout much of the year (Lohrenz et al. 1999, Rabalais et al. 1999). At present, there is no evidence that N_2 fixation is able to approach N requirements of these waters, even though they may be sufficient in the essential and potentially-limiting (to N_2 fixation) nutrient iron. Nitrogen fixation rates however are controlled by a complex array of environmental variables, including P and Fe availability, turbulence (high rates and persistent small-scale or mesoscale turbulence can negatively affect a wide range of N_2 fixing microorganisms), organic matter supply and grazing (c.f. Paerl 1990). Apparently, environmental conditions are not favorable enough for this process to supply all N needs in these waters. It is overly simplistic and unjustifiable to presume that "nitrogen fixation is efficient enough to ensure that marine waters are never nitrogen limited when there is an adequate supply of available iron" (Section 5.3.5 April 2004 report). There is no evidence that this reviewer knows of to support this statement.

The location and extent of zones of maximum primary producers relative to the hypoxic zones

The hypotheses and subsequent arguments about nutrient limitation and its ramification for productivity and hypoxia potentials conveyed in the various versions of the report rely heavily on knowing *when* and *where* the maximum productivity take place relative to the hypoxic zone in the MARDA and N. Gulf of Mexico. Furthermore, there is a great deal of uncertainty as to how, when, where and in what quantities "new" production is transported to the zones supporting hypoxic bottom waters. Clearly, vertical stratification is a prerequisite for bottom water oxygen depletion to take place. The potential for stratification is largely a product of freshwater discharge and local climatic conditions, both of which are relatively *uncontrollable*. A key *controllable* variable of hypoxia dynamics is the amount and extent of nutrient inputs controlling primary production and load new organic matter delivery to these regions. Therefore, to what extent specific nutrients and combinations of nutrients control primary production, specifically, the *maximum* production zone, are of central importance to understanding potential, appropriate and effective management actions that can be taken to reduce the magnitude, spatial and temporal extent of the hypoxic zone in the GOM. In this regard, it remains unclear where, when and how long periods of maximum primary production and phytoplankton biomass formation persists in the GOM (P. 12 of the report). This is a highly relevant and *critical* informational need, essential to knowing whether maximum productivity coexists with N or P limitation (or co-limitation), and whether the fate of nutrient limited productivity can spatially and temporally be linked to hypoxia dynamics.

From this report as well as previous publications cited in the report, it remains unclear *where* the zone of maximum primary production exists relative to nutrient inputs, their ratios and respective limitation potentials (P. 12 of the report). Temporal evidence linking periods of maximum primary production to periods of inferred (stoichiometrically-derived) nutrient limitation is clearer; this evidence indicates that springtime is the period of maximum primary production and this period co-occurs with very high N:P concentration ratios. The evidence presented in this report strongly suggests that during this period, P limitation may control new production, and recent bioassay-based evidence presented by Dortch et al. (submitted) tends to confirm this. It is less clear *where* the zone of maximum productivity exists during this period. Based on the data presented in this report as well as evidence in Dortch et al. (submitted), nutrient input constraints aimed at reducing primary production in the maximum zone will need to be considered on a seasonal basis.

Lastly, there is a time lag between the period of maximum primary production and bottom water oxygen depletion. Information is needed on how physical transport (i.e. the delivery of "new" organic matter) interacts with temporal lags in respiration rates (oxygen consumption) of the organic matter during the spring-summer transitional increases in water temperature (which at least in part control oxygen consumption rates), and the intensity of water column stratification.

Modeling efforts linking nutrient inputs to production and hypoxia dynamics

The linkages between nutrient inputs, light, primary production, temperature and water column stratification, and hypoxia dynamics are neither simple nor linear. These linkages (and the mathematical relationships describing them) are complicated by the distinct possibility that nutrient co-limitation or alterations between N, P and light limitation exist, that freshwater inflow delivering nutrients is also a strong determinant of stratification and hypoxia potentials, and that there may be significant spatial displacement and temporal lags between nutrient-enhanced primary production, phytoplankton biomass maxima and periods/zones of oxygen depletion. In addition, it is recognized that in the shallow waters of the MARDA and N. Gulf of Mexico, there are strong nutrient, carbon and oxygen interactions between the sediments and water column. These interactions have important ramifications for nutrient fluxes and availability (i.e. recycling), limitations and hence are likely to play an important role with respect to controlling primary production, mineralization, food web interactions and hypoxia dynamics. It is argued in the January 2004 and April 2004 versions of the report that the relatively "simple" sets of connected 2-D box models used to describe these interactions is inadequate and more sophisticated 3-D models are needed to more fully and quantitatively predict production and hypoxia responses to nutrient and hydrologic drivers. While improved connectivity and linkages between physical, chemical and biological drivers and processes determining and controlling hypoxia potentials is needed, it isn't clear, based on the content of this report, why 3-D models are absolutely (exclusively) needed to accomplish this task. While this reviewer is not an expert in modeling, a compelling case for such models, beyond the fact that the MARDA and the N. Gulf of Mexico waters are hydrologically and biogeochemically complex, was not made. Perhaps refinement of 2-D models with more well-defined and clearly-articulated linkages may be appropriate and achievable. It is suggested that before launching 3-D efforts, a panel of modeling experts be consulted to evaluate the modeling needs (based on the management needs). Clearer justification for the use (and expense of developing and operation) of 3-D models is needed (I can be convinced, but not by what is stated in the various versions of the report).

Critical Questions and Informational Needs
With respect to the abovementioned topics, critical questions, that previous work or the EPA reports do not clarify, should be addressed. These include:

1. Where *is* the zone of primary production and phytoplankton biomass accumulation in the MARDA? This is a critical to being able to evaluate the relative importance of N vs. P limited primary production as a source of "new" production (i.e. new C) as "fuel" for oxygen consumption and determining the magnitude and extent of hypoxia.

2. What is the importance of this "new" production relative to allochthonous organic matter (including terrestrial and riverine produced organic C sources)? Can this "new" production as well as terrigenously supplied C be traced as a source of hypoxia-generating COD/BOD using stable isotopes or other biogeochemical tracers?

3. The seasonal patterns of oxygen consumption and hypoxia formation are important. This information should be combined with question 1 in order to develop some predictive relationship(s) between nutrient loading and hypoxia formation/dynamics.

4. Is "excess" N really delivered to the coastal-shelf marine environment? In other words, is N loading currently so high to the GOM via the plume that the zone of maximum productivity cannot currently strip it all out before it enters the GOM proper and hence lead to stimulation of PPR in the coastal shelf region that exhibits hypoxia.

5. A physical-biological integrated model needs to be developed that can predict hypoxic volume, extent and duration interactive with and independent of nutrient enhanced primary production. Is such a model available? Are 2-D model applicable or are 3-D models justifiably needed? Stated differently, how complex do models need to be in order to realistically and usefully (for management purposes) capture the essential interactions between nutrient inputs, light, hydrodynamics, productivity and oxygen consumption/hypoxia dynamics.

6. What is the relative importance of autochthonously-produced vs. allochthonously supplied organic C in oxygen consumption and hypoxia dynamics of the GOM?

7. What is the temporal and spatial lag between nutrient-enhanced "new" production and hypoxia dynamics in the plume and GOM receiving waters? Can this be modeled? Can stable isotope and other tracer techniques be used to establish and confirm such lags?

8. To what extent can and should we solely rely on Redfield ratios to determine (and confirm) that either N or P or both are limiting PPr? It is likely that internal N and P cycling along the MARDA-GOM continuum strongly influences nutrient-production dynamics and Redfield ratio-based predictions of nutrient limitations. Parallel confirmation (of nutrient limitation and nutrient-productivity interactions) based on nutrient addition bioassays with natural phytoplankton communities under ambient light/temperature and grazing conditions, is needed as a more direct line of evidence for specific types of nutrient limitation and phytoplankton growth responses.

Reviewer 6

This document is a much-shortened version of the document originally released in January 2004 and slightly modified in April 2004. It presents information useful to the discussion of the causes of the Gulf Hypoxia problem, primarily the Mississippi and Atchafalaya River (hereafter referred to as river) data, but suffers from some problems and omissions. First, it is poorly referenced for a review on such a large well-studied topic. Second, the organization is unusual as the river data is presented in the results section and then additional northern Gulf (hereafter referred to as gulf) data is presented in the discussion section. In addition, little or no mention is made of the density stratification of the northern gulf waters and the significance of this stratification in hypoxia. This is a controversial issue that is believed by some to be very important in hypoxia formation. Another area of omission is the release of DIP from particulate P when river water meets seawater. Other important issues are also omitted (see below).

Reviewer 7

The August 2004 version of the Region 4 report (and the preceding April and January 2004 reports) relies centrally on what is termed in the report as "a more traditional Redfield ratio calculation." The use of this approach in the report is fatally flawed, rendering the report of no scientific use as a guide for policy. Most of my review will focus on how the Region 4 report mis-applied this concept. The rest of the Region 4 report is based on conclusions from this flawed mis-application, and does not stand on its own. Given these failings, I believe the Region 4 report should be formally withdrawn by EPA. There are several far more thorough and better supported analyses available to the policy community for guiding the Action Plan, and the Region 4 report obfuscates far more than it illuminates.

For preface, before getting into the details of how the Redfield ratio approach can and cannot be applied, I will note that the Region 4 report has very few references to the appropriate literature on the Redfield approach, and almost none on the rather large literature which addresses this approach in coastal waters. A striking omission is any reference whatsoever to the 2000 "Clean Coastal Waters" report of the National Academy of Science's Committee on Causes and Consequences of Coastal Eutrophication (NRC 2000). This report, which was funded by EPA, has a chapter specifically devoted to whether nitrogen or phosphorus is of more consequence to coastal eutrophication in the waters of the US, including the waters in the plume of the Mississippi River in the Gulf of Mexico. That chapter, while not exhaustive, is a reasonable starting point to read about relatively recent approaches to the use of the Redfield ratio as an indicator of nitrogen versus phosphorus control of eutrophication, as well as to other approaches for addressing this topic.

I also will note that I am a firm believer in the application of the Redfield ratio approach, when applied knowledgably. I studied with Alfred Redfield, and I have published extensively on how the Redfield ratio applies to nutrient limitation and eutrophication in coastal marine waters. My criticism here is not on the basic approach, but on its complete mis-application in the Region 4 report.

Use of the Redfield Ratio concept:

The fundamental premise behind the use of the Redfield ratio concept in the Region 4 report is that the ratio of dissolved inorganic nitrogen to dissolved inorganic phosphorus (DIN:DIP ratio) in the Mississippi River indicates the relative availability of nitrogen and phosphorus to phytoplankton in the Gulf of Mexico where hypoxia develops. <u>This premise is simply not true.</u> There are many potential problems with this analysis, some more subtle than others. The biggest flaw in the approach as applied concerns the phosphorus that is associated with particles (either adsorbed to particles or covalently bonded within the particles). Large amounts of particle-bound phosphorus come down the Mississippi River (see Sutula et al. 2004 for a recent estimate of the magnitude). The biovailability of this phosphorus would be low (probably very low) within the freshwater portions of the Mississippi River. But as the particles encounter the increasingly more saline waters of the Gulf of Mexico, the high ion abundances of seawater will cause virtually all of the adsorbed phosphorus to desorb, instantly converting it into highly bioavailable DIP (see reviews by Froelich 1988 and Howarth et al. 1995). Further, diagenetic processes that occur in marine sediments such as those in the Gulf of Mexico, but far less so in freshwater sediments (and not at all in the sediments of the Mississippi River itself; Sutula et al. 2004), can potentially release much of the phosphorus that is covalently bound within the mineral structures of the sediment, again making it bioavailable as DIP in the hypoxic zone but not in the freshwater Mississippi River. See Krom and Berner (1980), Caraco et al. (1989, 1990), Blomqvist et al. (2004), and Howarth and Marino (2005). Note that the importance of these process in coastal systems, as opposed to freshwater ecosystems, is also discussed in the NRC (2000) report. The best available evidence suggests that most of the particle-associated phosphorus that comes down the Mississippi River will become biologically available in the Gulf of Mexico (Sutula et al. 2004). Note that a sizeable fraction of the dissolved organic phosphorus is also likely to become available in the Gulf of Mexico, much more so that for dissolved organic nitrogen, which tends to cycle more slowly (NRC 2000). Therefore, the bioavailability of phosphorus in the Gulf is probably more accurately reflected by the ratio of dissolved inorganic nitrogen to total phosphorus in the Mississippi River. According to Figure 7 in the EPA Region 4 report, this value is typically around 10:1 (well below the Redfield ratio of 16:1), which would suggest that nitrogen and not phosphorus limitation would be more prevalent in the Gulf of Mexico as one moves out of the freshwater plume of the Mississippi River.

Another key factor is the role that coastal marine sediments play in recyling nitrogen and phosphorus in eutrophic environments (NRC 2000; Sutula et al. 2004; Howarth and Marino 2005). The phosphorus that comes down the Mississippi River is likely to be taken up many times by phytoplankton; each time it is taken up, some will be sedimented in organic matter to the bottom sediments, where most is likely to be diffused back to the water column. Nitrogen, on the other hand, is lost through this sediment recycling through the high rates of denitrification observed in coastal environments (Nixon et al. 1996). Thus, as water is advected along the continental shelf to the west of the Mississippi River, nitrogen is likely to become increasingly more limited. This alone is a critical reason for nitrogen control in the Mississippi.

The argument in the above two paragraphs is that the DIN:DIP ratio in the freshwater portions of the Mississippi cannot be used to estimate the relative availabilities of nitrogen and phosphorus downstream in the saline waters of the Gulf of Mexico. Can the DIP:DIN ratio in the Gulf itself be used to indicate whether nitrogen or phosphorus is more important in controlling eutrophication and hypoxia? Whether the DIN:DIP ratio can in general be used in this manner has been subject to quite some debate. See for example both NRC (2000) and Dodds (2003). Issues involve methodological biases, and the relative rate of recyling of nitrogen and phosphorus from organic pools. The key determinant in whether nitrogen or phosphorus is more controlling of eutrophication is the relative bioavailability of these elements relative to the Redfield ratio, and the DIN:DIP ratio is only a surrogate for this. The National Academy of Sciences' Committee on Causes and Management of Coastal Eutrophication (NRC 2000) addressed this issue, and concluded that when the DINM:DIP ratio is well below the Redfield ratio of 16:1, it probably does indeed indicate nitrogen limitation. On the other hand, ratios above the Redfield ratio may or may reflect phosphorus limitation: organic phosphorus recycles far faster than does organic nitrogen, which can allow nitrogen limitation even at DIN:DIP ratios well above the Redfield ratio. Figure 9 of the EPA

Region 4 draft report shows DIN:DIP ratios along “transect C sampling” as varying from 80 to 144 in January through May, and from 24 to 50 in June through December. In my professional opinion, the higher values that occurred from January through May are indeed likely to suggest phosphorus limitation along this sampling transect. On the other hand, the lower ratios between June and December are too close to the Redfield ratio to be interpreted in this manner, without much more information on the relative rates of uptake and recyling of these elements. Even the conclusion that phosphorus is probably limiting for the period of January through May should be treated with great caution, as this would only apply to the waters sampled. The EPA Region 4 report provides no data on the salinities along this transect at that time, although one can infer that salinities were low. This low-salinity condition would not be representative of the more saline waters in the Gulf hypoxic zone.

Other Comments on the Region 4 Report:

The Region 4 report almost completely ignores the large body of literature that deals with the importance of climatic variability on nutrient fluxes. There is a substantial literature, both in the Mississippi and generally in river basins, on this. See for example McIsaac et al. (2001) for a high visibility analysis of this effect on DIN fluxes in the Mississippi. For a broader analysis across the US, see Scavia et al. (2002). This topic is very well handled in the original CENR assessment, and the material presented in the Region 4 report ignores the pertinent science and adds no new information of value.

The Region 4 report is extremely light in its treatment of changes in sources of phosphorus to the Mississippi River basin (page 11). Factors not considered are the reduced use of phosphorus in detergents across much of the U.S. since the 1970s, and increased controls on phosphorus releases from industrial sources along the rivers, such as fertilizer factories and shipping facilities.

The Region 4 report refers to “EPA’s ecoregion-based nutrient criteria” (page 12). No mention is made of the EPA (2001) report on guidance for nutrient criteria in coastal waters, which is a far better treatment of this topic than were earlier EPA efforts in this regard. Are the Region 4 authors unaware of the EPA (2001) guidance document?

Conclusions of this Review:

The earlier CENR assessments, the Gulf Action Plan, and the NRC (2000) report from the National Academy of Sciences all called for control of both nitrogen and phosphorus to reduce problems in coastal waters. The reasons for this are that the scientific community has long known that phosphorus is sometimes limiting in coastal systems (either seasonally, or even year-round in some cases, when driven that way by extremely high nitrogen loads), and that phosphorus is generally limiting in lower salinity waters. Beyond that, phosphorus control in upstream freshwater ecosystems can have beneficial consequences not only in those freshwaters, but also in the downstream marine ecosystems (by increasing silica delivery for example; NRC 2000; Boesch 2002; Howarth and Marino 2005).

In this sense, the report of the Region 4 report is consistent with the vast majority of other recommendations for the Gulf of Mexico. However, the logic behind the Region 4 report is fatally flawed. The report adds nothing of value to the scientific underpinnings for remediating nutrient pollution in the Gulf of Mexico. Nor is it likely that the report can be “fixed” by simple additions, deletions, or other editing. I view this report as a large mis-use of scarce resources toward the application of quality science to solving societal problems.

Reviewer 8

As an introductory remark, I feel it is important to acknowledge the complexity of the problems being addressed. All the documents have both merits and flaws. However, the fundamental issue of whether phosphorus, as well as nitrogen, needs to be considered in management strategies is common among all the documents. The documents raise serious concerns about a management strategy focusing solely on a single nutrient element. I contend that given the complex nature of the problem and limited understanding of many issues, only further monitoring coupled with an adaptive management approach will resolve some of the questions raised. Such a management approach should entail an extensive ongoing program of monitoring both river and shelf water properties, combined with a comprehensive program of modeling that will both enhance understanding of mechanisms and allow for prediction of outcomes in response to different management actions.

An overarching concern to reviewer is that the contentious nature of the documents and this review process will provide ammunition to special interests to further postpone actions to mitigate nutrient overenrichment occurring in the Mississippi River Basin. The dispute over the role of phosphorus versus nitrogen does not dispel the fact that both nutrients are initially discharged at levels far in excess of saturating levels for nutrient uptake by phytoplankton. There is NO DOUBT that excessive introduction of nutrients does degrade the overall quality of coastal zone impacted by river outflow. The Mississippi River discharges levels of dissolved inorganic nitrogen comparable to that of the largest river in the world (Amazon River), yet it has roughly only a tenth of the freshwater discharge. Case studies in other coastal systems throughout the world provide clear evidence of the potential consequences of lack of action to reduce eutrophication.

There is no simple solution. Action must be taken to reduce BOTH nitrogen and phosphorus in receiving waters of the Mississippi River. Monitoring and modeling must occur in parallel with nutrient reduction efforts, and follow an adaptive approach that will ensure that mitigation efforts are responsive to observed changes (or lack of changes) in conditions. Finally, further study is needed to better understand linkages between nutrient inputs, algal productivity, carbon fluxes to bottom waters, and hypoxia. Such studies should be carried out in parallel with nutrient reduction and monitoring efforts.

Review of “Review of Issues Related to Gulf of Mexico Hypoxia,” January, 2004

This document is the most detailed of the three documents, and highly critical of the Final Integrated Assessment developed from a series of Committee on Environment and Natural Resources (CENR) reports. The Final Integrated Assessment was used as the basis for a proposed Hypoxia Action Plan (**http://www.epa.gov/msbasin/actionplan.htm**) calling for a 30% reduction of total nitrogen in the Mississippi River Basin.

The January 2004 document re-examined data presented in the CENR reports and introduced other nutrient data from the lower Mississippi and Atchafalaya rivers and the northern Gulf of Mexico. The overall conclusion of the document was that there is a lack of compelling evidence to justify a nutrient reduction strategy that focuses solely on nitrogen. Indeed, the results presented do raise serious questions regarding such an approach. *However, the document goes too far in arguing solely for a phosphorus reduction approach.* Notably, the report draws attention to uncertainties in our understanding of nutrient control of productivity, and linkages between algal productivity and hypoxia. The document calls for additional investigation and monitoring, which are certainly valid points.

Concluding Statements

In my review of the EPA Region 4 documents, I have attempted to give consideration not only to these documents, but also to a number of the key references cited. For the most part, I have been impressed with the effort of the authors of all these documents and recognize that my efforts in this cursory review pale in comparison to the time and intensity invested in these various documents. The one exception to this that puzzles me is, given the large amount of information in the CENR Reports and literature, how did we end up with a Hypoxia Action Plan based on management of the single nutrient, nitrogen? To quote from the Rabalais et al. (1999) CENR report:

> "Managing for a single nutrient is difficult for the large Mississippi River system because N and P may change together but not linearly, and Si is complexly interrelated with P dynamics and/or water retention in the watershed."

Similarly, from Brezonik et al. (1999):

> "Differences in results [increases in average dissolved oxygen concentrations] between reductions in N and P loadings were generally not significant…"

I am even more perplexed at the tenaciousness at which individuals who are obviously knowledgeable seem to have clung to the single-nutrient management approach when there is obvious evidence that both nitrogen and phosphorus play key roles in excess primary production, and both nutrients can potentially limit algal production. To conclude, I believe there is ample evidence to justify responsible, balanced, and comprehensive management strategy addressing both N and P. An adaptive management approach is essential. Lack of action will maintain, or worsen the status quo, with dire consequences for our coastal waters.

Reviewer 9

The fundamental recommendation to reduce nitrogen (N) and phosphorus (P) loading to the Gulf of Mexico "there may be a considerable benefit to reducing both nutrients in order to restore water quality" that is reached in this document is sound. However, the scientific reasons for reaching this recommendation as cited in this report could be strengthened.

The reasons I would support this recommendation are: 1) the data suggest either P or N may be limiting planktonic production as the Mississippi basin water enters the gulf, 2) the Gulf is clearly N limited, so even if there is a gradient from P to N limitation as water mixes with the higher salinity Gulf water, ultimately N fertilization is a problem, and 3) research on N and P limitation from freshwaters (Dodds et al. 2002) has demonstrated that chlorophyll yield can be higher for each unit of N if there are low N:P ratios (i.e. lots of P relative to N) and chlorophyll yield can be higher for each unit of P if there are high N:P ratios (i.e. lots of N relative to P).

While much of the research cited in this peer review is from freshwaters, there is good evidence that there are not substantial differences between N and P limitation responses by phytoplankton in fresh and marine waters, and that total nutrient concentrations can be used for stoichiometric analysis in similar ways in both marine and freshwaters (Guildford and Hecky 2000).

IV. RESPONSE TO CHARGE

1. *Do the Data Presented Provide Sufficient Evidence for P Limitation of Phytoplankton Growth?*

Reviewer 1

There is sufficient evidence to support the limitation of phytoplankton growth and thereby hypoxia in the interstitial waters of the northern Gulf. In these waters, salinity changes seasonally and can support P rather than N limitation of algal growth. In the larger context of the Gulf overall, however, complex and interrelated process combine to confound biological growth limitation by one nutrient or the other. Bottom sediments may become anoxic and release P to overlying interstitial waters, which could become a source of P to algae. In other areas, N is limiting growth. Having said this and reiterated the obvious that both N and P can limit phytoplankton growth in the Gulf at various in a spatially and temporally interdependent manner, control of nutrient loss to the Mississippi and Alchafalaya Rivers and input to the Gulf, should focus on both elements. Unless, both nutrients are addressed in the source watersheds and basins, then an effective and widespread decrease or management in hypoxia will not occur in the larger northern Gulf waters.

Reviewer 2

No answer provided.

Reviewer 3

Not in any sense important to understanding the causes of and solutions to hypoxia. While it has been well recognized (e.g., Rabalais et al., 1999) that phosphorus (P) may limit the growth of phytoplankton at a given time and place (winter-spring and lower surface salinities), the data and arguments offered in support of the case for phosphorus limitation of production—over the appropriate time and space scales that influence hypoxia—are fatally flawed for the following reasons:

a. over-reliance and misuse of the DIN:DIP ratio (see 8 a),
b. substantial underestimation of bioavailabile P loadings (see 8 b),
c. failure to address seriously the abundant, contradicting evidence for nitrogen control of production that drives hypoxia, including the strong interannual and longer-term relationships among nitrate loading, production and hypoxia (see 2), and
d. inadequate consideration of the dynamics of production, nutrient cycling, and hypoxia formation and maintenance in the realistic context of the Louisiana continental shelf (see 10 b).

Reviewer 4

Rabalais et al. (1999) had already presented convincing evidence and discussion of P limitations contributing to hypoxia. So this is not a controversial point. What might be controversial is the degree to which N and P contribute to hypoxia, but the Region 4 report makes no attempt to quantify this.

Reviewer 5

No answer provided.

Reviewer 6

The river and gulf data presented and referred to are suggestive of phosphorus (P) limitation, especially in the zone just west of the Mississippi Delta, but do not provide convincing evidence for it. The river DIN:DIP ratios (Fig. 6), especially in the spring, suggest a potential for P limitation but do not prove it. Likewise for the N:P elemental ratios along the C transect (Fig. 9). Direct evidence of limitation from nutrient addition experiments, like the Smith and Hitchcock, 1994, paper cited in the report, are needed to demonstrate nutrient limitation, especially when coupled with other measurements, such as particulate elemental ratios, as well as molecular or enzyme measurements, along with measurements of nutrient concentrations and ratios. P may be a very important limiting nutrient in this system and have a major role in controlling hypoxia, but the evidence given in this report is just a first step in the process.

Reviewer 7

No, the report relies excessively on a mis-application of the Redfield ratio concept, and a re-analysis of the data (considering the increased biovailability of particle-associated phosphorus once it reaches saline waters) leads to the opposite conclusion: nitrogen is more likely to be limiting. However, there is a large body of evidence out there on both phosphorus and nitrogen limitation – poorly handled in the Region 4 report – and it in fact seems likely that phosphorus is limiting in some places and times, particularly in the low-salinity waters in the spring (as is commonly seen in most coastal waters; NRC 2000).

Reviewer 8

The critical question is not whether there is sufficient evidence for phosphorus limitation. Rather, is there justification for only managing nitrogen? And, no there is not. The CENR reports and the EPA Region 4 documents all provide ample evidence that phosphorus may limit primary production in the Mississippi River outflow region, particularly at key periods of high river discharge and high productivity, and in regions of chronic hypoxia. The location of potential phosphorus limitation is generally at intermediate to low salinity regions. However, there is also evidence for nitrogen limitation, generally at higher salinities and during low discharge.

Reviewer 9

The data presented are insufficient to provide evidence for sole P limitation in the Northern Gulf/ Discharge area. They are also insufficient to rule out P limitation. The literature evidence reviewed in this report suggests that at times, P can be limiting or co-limiting with N. The problems come with a primarily incorrect premise that ratios of dissolved inorganic nitrogen (DIN, nitrate + ammonium) to dissolved reactive phosphorus (also referred to as soluble reactive phosphorus, SRP, in the literature) can be used to reliably indicate N or P limitation. There are several lines of evidence to suggest that DIN/SRP ratios are not reliable indicators of nutrient availability (Dodds 2003). These will be reviewed here briefly, and additional information also will be discussed.

The assumption that relative concentrations of inorganic nutrients can be used to determine relative supply rates (availability) to primary producers (page 1 paragraph 2) is based upon 2 basic fallacies: 1) It is incorrect that SRP values represent phosphate concentration. This has been demonstrated to be false repeatedly (e.g. Hudson et al. 2000). 2) It is not necessarily true that the size of a dissolved nutrient pool is directly proportional to the rate that it can be used. Even though phosphate concentrations can be extraordinarily low, uptake demand can be very high because the half saturation constant for uptake is very low (Dodds et al. 1989). SRP is a poorly defined chemical fraction, and the amount of phosphate can vary as a function of P limitation. In general, actual phosphate concentrations are far lower than the SRP values (Dodds 1995). Nutrient pools are highly dynamic and turnover rate is important as well as absolute concentration (Dodds 1993). For example, in highly eutrophic systems, SRP and DIN values can be very low at times because algal demand is very high.

Probably neither nutrient is limiting as the water enters the northern Gulf of Mexico because the absolute concentrations of dissolved inorganic N and P are so high. These values are well above the half saturation constants reported for phytoplankton assemblages (e.g. Dodds et al. 1989).

The statement in the second paragraph on page 17, that DIN:SRP ratios that are 2-3 times greater than Redfield ratios indicate potential for P limitation is not well supported. It is possible, for example, that luxury consumption of P leads to very high cellular contents of P and that from that point forward N, not P limitation is what controls primary production. Physiological data, including particulate C:N:P ratios would help in determining if this were the case.

2. *Do the Data Presented Provide Sufficient Evidence for N Limitation of Phytoplankton Growth?*

Reviewer 1

There is again, sufficient information to support N limitation of phytoplankton growth in the northern Gulf. However, there is not sufficient information as for P, to show that this is so widespread and dominant as to be the only nutrient of concern in the Gulf. There are areas of the Gulf (interstitial less saline waters) and times of the year, when P in likely the nutrient limiting phytoplankton growth.

Reviewer 2

No answer provided.

Reviewer 3

The Region 4 reports fail to acknowledge, much less refute, the diverse body of evidence that supports the CENR conclusion that increased nitrogen (N) loading is the principal determinant of primary production and cause of worsening hypoxia. With its incessant, narrow focus on DIN:DIP ratios, the Region 4 assessment is a one-trick pony in a multi-ring ecosystem circus. Specifically, the reports do not address: (a) the large literature on marine eutrophication that provides evidence and rationale for the dominant importance of N loading on primary production and hypoxia intensification in temperate coastal ecosystems (NRC, 2000); (b) observations that inter-annual variations in primary production in the Gulf are strongly related to inputs of N (e.g., Lohrenz et al., 1997); (c) the longer-term course of events in the northern Gulf of Mexico in which it has been demonstrated that hypoxia intensified coincident with increased nitrate loading (Rabalais et al., 1999, and many other papers by these authors); and (d) the skill of models that hindcast and nowcast the extent of hypoxia based on N loading (Scavia et al., 2004). In neglecting to consider this diverse evidence, statements in the earlier draft reports such as "There is a lack of compelling evidence that reduction of nitrogen would reduce the supply of organic matter fueling hypoxia" and "evidence . . . do [sic] not support arguments that a 30% reduction in total nitrogen would have an impact on hypoxia in the Gulf" do not meet professional standards of due diligence in challenging one's conclusions based on <u>all</u> the evidence.

Reviewer 4

No, this was not a focus of this paper.

Reviewer 5

No answer provided.

Reviewer 6

There is little in this report suggestive of nitrogen (N) limitation, nonetheless, N limitation cannot be ruled out, especially in the late summer-early fall period of river flow, when DIN:DIP ratios in the river and gulf are at a minimum. N is the default assumption for the limiting nutrient in most coastal regions. It is likely that the gulf was primarily N limited before the major influx of anthropogenic N and P. In addition, most statistical correlations with the area of hypoxia work better with N than P river inputs, at least partly because there is more river N data. Assuming that all river nutrients are used in the gulf, N inputs may control the ultimate amount of phytoplankton primary production possible.

Reviewer 7

Again, no, the report is fatally flawed, and can shed no light whatsoever on the key issues.

Reviewer 8

See the answer to number 1 above, and the discussion elsewhere in this review. Yes, there is evidence for nitrogen limitation, although it tends to be more likely during late summer and at higher salinity regions. Without understanding linkages of where and when organic matter fueling hypoxia is produced, it is not feasible to discount either nitrogen or phosphorus as critical limiting nutrients.

Reviewer 9

The data are insufficient to provide adequate evidence for sole N limitation as well as being insufficient to rule it out. The literature data provided make a case for N limitation occurring at times, potentially a co-limitation with P as well. Please see comments above for my rationale.

3. *Do the Data Presented Support the Conclusion That Point Source P Contribute to Algal Blooms and Gulf Hypoxia?*

Reviewer 1

There is sufficient data to support the conclusion that point sources of P contribute to phytoplankton growth in the northern Gulf. Having said that, there is also a wealth of information to show that nonpoint sources in the Mississippi and Alchafalaya River Basins, such as from metropolitan, urban, suburban, rural, and agricultural activities are major sources of P to the Gulf. Although concern over hypoxia in the Gulf is not new, there has been a profound shift in our understanding of, and focus on, sources of P and N in water bodies. Since the late 1960s, the relative contributions of these nutrients to water bodies from point and non-point sources have changed dramatically. On one hand, great strides have been made in the control of point source discharges of P, such as the reduction of P in sewage treatment plant effluent. These improvements have been due, in part, to the ease in identifying point sources. On the other hand, less attention has been directed to controlling non-point sources of P, due mainly to the difficulty in their identification and control. Thus, control of non-point sources of P and N is a major hurdle to protecting northern Gulf waters from increased algal growth and hypoxia.

Reviewer 2

No answer provided.

Reviewer 3

The earlier drafts (January and April) of the report present comparisons between DIP concentrations and loadings at St. Francisville and Belle Chasse (downriver) to suggest that, because the DIP loadings are higher downriver, discharges from point sources are a quantitatively significant portion of reactive P loadings to the Gulf. In the latest (August) draft the analysts drop this comparison for some unknown reason. Nonetheless, when these observations are taken together with an inventory of industrial and municipal point source discharges (see 7), it appears that downstream discharges comprise a non-trivial component of reactive P delivered to the Gulf. However, for reasons discussed under 10, this does not necessarily mean that this point-source P contributes significantly to algal blooms that cause hypoxia.

Reviewer 4

No. The only data provided on sources of P was fertilizer use in the USA, which is mostly a non-point source and only a partial picture.

Reviewer 5

No answer provided.

Reviewer 6

No. A prior version of this report (April 2004) showed a significant increase in DIP loads between St. Francisville and Belle Chasse, suggesting major P inputs, probably point source, in that limited distance. However, the St. Francisville data has been removed from the current (August 2004) report and just the Belle Chasse DIP concentrations remain. This report is suggestive of P limitation, as described above, which suggests that P inputs (point source or non-point source) are potential contributors to algal blooms. However, this current report provides no information as to the type of P sources (point or non-point) important to the system.

Reviewer 7
No, although this may be the case. This report is too flawed to provide any insight on this question.

Reviewer 8
In general, point sources appear to be a relatively small fraction of total nutrient inputs to the river basin (Goolsby et al., 1999). The results shown in the January and April 2004 Region 4 documents provide some evidence for higher concentrations of DIP at Belle Chase relative to St. Francisville USGS monitoring stations over the period of 1980-1999. Based on findings given in the Knecht (2000) report, this might be explained by point sources of phosphorus released prior to 1994. These sources appear to have been reduced since 1994. Nevertheless, such findings are ample justification for maintaining nutrient monitoring activities in the lower river.

Reviewer 9
The data here do not distinguish between point source and non-point source origins of P loading to the Gulf of Mexico. A much more complete accounting would be necessary to accomplish this.

4. *Is the Observed Trend on P Loading Consistent with Gulf Reactive P Increase?*

Reviewer 1

I believe so. One can always question the frequency, location and timing of sample collection for nutrient analysis, however, it would be obviously impossible to get a complete nutrient concentration, reactivity and load in time and space. Thus, we end up doing the best we can within our usually limited resources. I believe we have now passed the stage where we always recommend that more information is needed. What is needed is to look at the mechanisms whereby necessary changes in land management, nutrient use, growth, and a dwindling of natural ecosystems within the Mississippi and Alchafalaya River Basins can be made.

Reviewer 2

No answer provided.

Reviewer 3

No. Rabalais et al. (1999) did not "report" such an increase based on actual data because P concentrations were not routinely monitored prior to 1973 and very few measurements of P concentrations on the Gulf shelf were made prior to the 1980s. Rather, they present a table from several of their earlier papers (e.g., Justić et al., 1995) in which they <u>speculated</u> about the potential significance of shifting ratios of nutrients delivered to the river from the past, to the present, and into the future on phytoplankton production in the northern Gulf of Mexico. In these papers the authors <u>estimated</u> that total phosphorus (TP) concentrations in the river had been nearly one-half lower in the early 1960s based on linear extrapolation of 1973-1987 trends back in time. They further "reconstructed" RP concentrations in the Gulf 1960s by <u>assuming</u> that they were lower in the same proportion as the TP estimates for the river. Obviously, whatever errors may be associated with the river TP estimates would be compounded by errors in this extremely simple assumption concerning a highly non-conservative property, such as reactive P.

The Region 4 authors at one point seem to understand that these estimations do not, in fact, constitute evidence of increased loading of P, stating: "However, the report [Rabalais et al., 1999] concluded that there was insufficient phosphorus data to statistically calculate the relative change in load with respect to time" (August draft, p. 11). Nonetheless, they seem to accept Justić et al.'s (1995) string of speculation as fact, stating "the observed trend in phosphorus loading appears to be consistent with the two fold increase since 1960 in reactive phosphorus in the Northern Gulf" (p. ii). But, there was no <u>observed</u> trend, nor is their any evidence of two-fold increase in reactive P measured in the Gulf! Rather, the reconstructed RP concentrations in the Gulf, by definition, track the total P estimates in the river from which they were mathematically derived. The reasoning is both implausibly hypothetical and totally circular.

In sum, there is no reliable (methodologically comparable and with sufficient sampling to account for spatial and temporal variability), direct evidence of changes in the river TP or Gulf RP concentrations over this time period. In fact, Goolsby et al. (1999) found "no apparent long-term trend in either ortho P or total P concentrations . . . in the Mississippi River at St. Francisville since the period of record began in the early 1970s," calling into question the validity of linear extrapolation from the 1973-1987 measurements to estimate 1960-62 concentrations.

Why would one expect P concentrations to have doubled from 1960 to through the 1980s? Justić et al. (1995) cited the doubling of P-fertilizer use during that time period (Turner and Rabalais, 1991) as a line of evidence supporting their estimation of 1960-era concentrations. But, because fertilizer-P is estimated to contribute only 31% of the flux in the 1990s, doubling of this component of the loading would have resulted in only an 18% increase in total P loading during the period, all other things being equal. Furthermore, Goolsby et al. (1999) observed: "One can hypothesize that the flux of P to the Gulf was considerably higher prior to completion of the Missouri River reservoirs in the 1950s that it is today." That is because most of the P transported is adsorbed to sediment particles, which were increasingly trapped behind dams. In fact, Kessel (1988) showed a much greater than 50% reduction in suspended sediment load in the lower River after 1950, nearly a 50% reduction from 1958-62 to the 1980s. Taken together, these two up-basin drivers (fertilizer use and sediment trapping) call into question any assumption of a two-fold increase in P concentrations and loadings, at least as observed at St. Francisville.

While P loadings from industrial point source discharges below St. Francisville undoubtedly increased since the 1960s with the increase in fertilizer production, these should not have increased total P loadings to the Gulf by more than 19%. Furthermore, these P loadings appear to have declined by two-thirds since 1993 (Knecht, 2000; see 7, below).

Reviewer 4

What trend in P loading data? The fertilizer use data for the US in Figure 8? There is an approximate 2 fold increase in both variables, but there are so many factors that influence P movement, not to mention other forms of P, and there is uncertainty about that 1960 concentration value in the Gulf, so that little can or should be made of the similar magnitude of change. Or by P loading data, do you mean the P concentrations in the River entering the Gulf, where no trend was identified in the 1979 to 1998 period. But this is not inconsistent with the reported increase by Rabalais et al. (1999) because the data cover different time periods.

Reviewer 5

No answer provided.

Reviewer 6

Rabalais et al. (1999) found a two-fold increase in total P in the lower Mississippi River between 1973-1987. In a comprehensive search of that paper I found no mention of a similar increase in reactive P in the northern gulf, though I may have missed it. Such an increase is possible or even probable, but I have not seen the evidence for it. In addition, the period of time for the increase must be specified, as there is significant variation with much of the increase in river nutrients occurring by the 1980s. The limited river ortho-P (reactive P) data presented in this EPA report is all from 1980 or later, likely to be past any period of major increase. The data as presented (Figs. 3 and 5) is rather limited and variable, and shows few strong trends. If anything, there has been a decrease in Belle Chasse river P since 1980 (Fig. 3), and the Morgan City P concentration shows little or no change (Fig. 5), though the minimum concentration has declined.

Reviewer 7

No. A much more thorough analysis of the climatic controls on fluxes would be required, and one would need to thoroughly consider the inputs of particle-bound phosphorus.

Reviewer 8

Figure 5.6 in Rabalais et al. (1999) shows a substantial increase in phosphorus loading to the river between 1970 and 1990. Figure 5.14 in this same reference shows an inferred increase in surface reactive phosphorus concentrations in the northern Gulf of Mexico, but the historical 1960 values (from Justic et al., 1995) were reconstructed by “assuming that the relative proportion of nutrients in the river-dominated coastal waters reflects changing composition of riverine nutrients” (Rabalais et al., 1999). The fact that the authors had to resort to such a reconstruction method is a clear demonstration of the need for expanding and maintaining nutrient monitoring in the river and river-impacted shelf waters.

It should be noted that one might not necessarily expect there to be a correlation between nutrient inputs and surface concentrations, particularly if nutrients are rapidly subject to biological uptake (e.g. Lohrenz et al., 1997). So the point of this question is not entirely clear.

Reviewer 9

This is a difficult question. While P loading from fertilizer application has increased, so has human population and P effluent from point sources. Since the accounting is not complete, the trend in increased P loading is consistent with the increase in SRP, but it would be much stronger if total loading from point and non-point sources were quantified and related to total P rather than SRP concentrations.

5. *Are There Other Mechanisms That Cause the N and P Increase in the Gulf?*

Reviewer 1

There are many factors that contribute to increased P and N in the northern Gulf. These range as noted from the loss of natural wetlands, which are known and have been shown to be an effective sink of nutrients and mechanism by which nutrient loading is decreased; to hydrologic changes, and shifts in agricultural production systems. With the loss in wetland area, there will be a concomitant decrease in P and N retention within the Mississippi and Alchafalaya River Basins and thereby increase in nutrient loading to the northern Gulf. Similarly, changes in river channel morphology to improve river navigation, as well as land use changes, have all contributed to an increase in the hydrologic response of the Mississippi and Alchafalaya River Basins to rainfall inputs. In other words, the water cycle has become more dynamic and responsive to climate and as a result, the degree to which nutrient loss can occur and bypass natural retention mechanisms has increased. Land use changes in the Mississippi and Alchafalaya River Basins has led to a certain extent to a decrease in the nutrient retention times in the Basin.

While a variety of non-point sources, ranging from suburban lawns to construction sites to golf courses, contribute P and N to the Mississippi and Alchafalaya River Basins, significant changes in agricultural production systems, have occurred in nutrient management in agricultural systems since the early 1950's, which have influenced the usage, cycling, and accumulation of N and P in the landscape and increased the risk of nutrient loss from land to water and air. Before World War II, for example, sustainable nutrient cycles existed within farming communities, where enough feed was produced locally to meet animal requirements and manure could be recycled to local soils to meet crop nutrient needs. After World War II, fertilizer production and distribution became cheaper; N use increased four-fold (8.5 million tonnes) and P use two-fold (0.9 million tonnes) between 1950 and 1990 (Evans et al., 1996).

With the advent of new technologies, mechanization, increased chemical use, and government incentives, agricultural production has more than doubled and become concentrated on less agricultural land and on fewer, but larger, farms (Evans et al., 1996). Between 1950 and 1990, U.S. farm land has decreased from 1200 to 970 million acres (20%) and the number of farms from 5.6 to 2.1 million (63%), while average farm size has increased from 213 to 469 acres (120%). Also, farming systems have become more specialized, with crop and animal operations efficiently coexisting but in separate regions of the country, as seen by the switch from crop- to animal-based systems in several important agricultural states (Kellogg et al., 2000). This has led to a large-scale, one-way transfer of nutrients from grain- to animal-producing areas and dramatically broadened the emphasis of nutrient management and water quality remediation strategies from field and watershed levels to national scales.

As a result, agricultural systems have evolved from net sinks of P and N, where nutrient deficits can limit crop production to net nutrient sources, where P and N inputs in feed and mineral fertilizer can exceed outputs in farm produce. Over the last 50 years for example, some 600 tonnes of P were applied to agricultural land worldwide compared with about 250 tonnes of P removed as produce. The trend of increased fertilizer use in crop production over the last 50 years has fragmented farming systems, creating specialized crop and animal feeding operations that efficiently coexist in different regions within and among countries. During the last 10 years in the U.S., cattle, pig, and poultry numbers have increased 10 to 30%, while the number of farms on which they were reared has decreased 40 to 70% (Gardner, 1998).

This intensification has been driven by a greater demand for animal products and an improved profitability associated with economies of scale. Intensification has also resulted in a major one-way transfer of nutrients from grain-producing areas to animal-producing areas. This is occurred to the marked economic success of crop and livestock production systems in the Mississippi and Alchafalaya River Basins.

As it is cheaper to treat the cause of any water quality impairment rather than its effects, there has been widespread a “u-turn” in strategic planning to address water quality impairment. For example, in the early 1990s New York City decided that it was more cost-effective to identify the sources of P and N in its water supply watersheds and target them for remediation, rather than build new water filtration facilities. Since then, a variety of measures have been implemented to reduce nonpoint nutrient sources in the New York City watershed (National Research Council, 2000b). Similarly, there is increasing awareness within Europe that installation of expensive P stripping facilities at wastewater treatment plants, as required under the European Community Urban Waste Water Directive (Council of the European Communities, 1991), will not provide the desired improvement in water quality without management of nonpoint sources in sensitive watersheds. This is also the case for the Mississippi and Alchafalaya River Basins.

Reviewer 2
No answer provided.

Reviewer 3
As addressed under question 4, there is no observational evidence that phosphorus loadings or concentrations have increased coincident with worsening hypoxia. Some factors likely decreased P loadings (reduction of suspended sediments), while others (increased fertilizer use and industrial discharges in the lower river) probably increased P loadings. In any case, considering the scale of these changes, a doubling of P loadings since 1960 is far-fetched. In contrast, there is strong documentation that loadings of nitrate-N nearly tripled between 1955-70 and 1980-96 (Goolsby et al., 1999; CENR, 2000). Rabalais et al. (2002) considered alternate explanations of worsening hypoxia, including coastal wetland loss and organic carbon loading. No plausible mechanism or rates of supply have been offered whereby coastal wetland loss could provide quantities of either labile organic carbon or nitrogen that would approach *in situ* phytoplanktonic production or riverine nitrogen loading, respectively. On the other hand, changes in river basin hydrology (particularly artificial drainage of crop lands) and agricultural intensification, including the 6-fold increase in the application of fertilizer nitrogen, are thought to be the principal cause of worsening of hypoxia since the 1960s (CENR, 2000).

Reviewer 4
Mechanisms other than what? Fertilizer use or point sources? If fertilizer use and point sources are the primary mechanism proposed, then yes, surely there are other factors influencing P concentrations in the River and Gulf such as climatic change (increased precipitation and stream flow between 1960s to 1980s), changes in farming practices (e.g., concentration of livestock production), drainage of wetlands, and expansion of artificial drainage in agricultural regions. These were all discussed inthe CENR assessments.

Reviewer 5
No answer provided.

Reviewer 6

Assuming that there has been an increase in N and P in the gulf, river loading is the most obvious culprit. Nonetheless, other mechanisms, such as those mentioned, could also be important. River flow has increased in the last few decades (Raymond and Cole, 2003), which probably contributes to the increased nutrient flux. I am not really in a position to evaluate all the other possible mechanisms, it would seem that loss of wetlands and changes in agricultural practices could have some effect. However, I would still suspect that river loading would be the major contributor to any such gulf nutrient increases.

Reviewer 7

Yes. One prime factor would be the tendency for increased phosphorus return from sediments as the coastal system becomes more eutrophic. See NRC (2000) and Howarth and Marino (20005).

Reviewer 8

Of course, such alternative explanations cannot be ruled out. However, the principle of Occam’s Razor is that “Entities should not be multiplied unnecessarily." There is no doubt that loading from the river has increased. Certainly other explanations for observed changes in surface water need to be considered, but it would seem that river loading is the first place to look. The additional factors mentioned above must be considered along with river loading, and will require rigorous study, modeling, and monitoring.

Reviewer 9

This is also a difficult question. Of course changes in hydrology and agricultural processes will influence N and P loading to the Northern Gulf. I am not sure what this question is asking; mechanism others than what?

Hydrologic alteration related to channelization and decrease in riparian and delta wetlands may cause decreases in deposition, decreases in denitrification rates, and increases in loading to the Gulf. However, increases in the number of impoundments in the Mississippi basin may increase retention. Changes in agricultural practices can range from a drastic decrease in fertilizer use, to terracing and restoration of riparian vegetation. It is impossible to answer the question as framed given the lack of information available at the scale of the Mississippi basin, and the non-specific nature of the question.

6. *Do the Presented Data Support N and P Reduction Goals on a Seasonal Cycle?*

Reviewer 1

I believe information has been presented which shows that nutrient loading to the northern Gulf from the Mississippi and Alchafalaya River Basins varies seasonally. Thus, reduction goals and strategies to achieve them should be seasonally flexible to reflect major shift in P or N loadings. As a great deal is know about the inputs, dynamics, cycling and fate of P and N in the Mississippi and Alchafalaya River Basins in general and in agricultural production systems in particular, it should not be difficult to develop, target and implement remedial strategies in the source basins that are reflective of seasonally-dependent nutrient fluxes.

Reviewer 2

No answer provided.

Reviewer 3

It does not appear possible that N and P loadings can be controlled on a seasonal basis, so I assume that the question pertains to the Region 4 reports' assertions that the late winter-spring nutrient loads (when DIN:DIP in the river is typically high) control hypoxia. From that the authors conclude (in the January and April drafts, at least) that efforts to reduce hypoxia by reducing N loads would be futile, unless load reductions in excess of 70% were achieved. There are numerous problems with that line of reasoning including: (a) underestimation of bioavailable P through the use of only DIP (see 8), (b) dominance of nitrogen limitation in waters overlying hypoxic waters far removed from the dilution plume (see 10b), (c) recycling of nutrients over many months after their discharge (10b); and (d) the importance of continued surface-water production in sustaining hypoxia during the summer (10b). Having said that, efforts to model the extent of hypoxia have, in fact, already focused on spring (May-June) N loading as a key determinant and have estimated that a 40-45% reduction would achieve Action Plan goals for hypoxia reduction (Scavia et al., 2003).

Reviewer 4

Possibly, but again, the seasonality of the processes has been known for a long time. There may be some value to selectively reducing point source N and P discharges at critical times of the year, but this is not discussed in any detail in the Region 4 report. For non-point sources, the release of N and P is influenced by weather and there are multi year lags that complicate attempts to limit seasonal N and P releases. None of this is discussed in the Region 4 report.

Reviewer 5

No answer provided.

Reviewer 6

Based on the data presented here and that in the literature, P limitation is more likely in the spring and early spring and N limitation in the late summer and fall. Therefore, there may be an advantage to reducing P prior to the spring and N prior to the fall. There is a lag time of a month or two between the release of nutrients to the river and the effect of these nutrients on gulf phytoplankton production. This also assumes that nutrient limitation in the gulf actually corresponds to the river loading of P and N (see Questions 1 and 2 above).

Reviewer 7

No, the Region 4 report is too flawed to draw any such conclusion. Instead, policy makers should draw on the far more substantial analyses presented in the CENR assessments (which I might note received extensive peer review, before being introduced to the political world; this is a sharp contrast with what has happened with the various drafts of the EPA Region 4 report).

Reviewer 8

Absolutely. There is clear evidence in the EPA Region 4 documents and citations within that nutrient stoichiometry and loading is seasonally variable. DIN:DIP ratios tend to be higher in the spring during periods of high discharge, and decrease towards late summer and fall. These findings have clear implications for management strategies.

Reviewer 9

The data presented here suggest that goals could depend on a seasonal cycle, but total N and P seasonal loadings would make a stronger case. If fertilizer runoff is a primary culprit, the season of fertilization is mainly late spring, so seasonal goals may not mean much since the highest concentrations coincide with the highest runoff and the time of year when farmers tend to fertilize fields the most heavily. Also, determination of seasonal effects would require understanding of the time-lags between nutrient enrichment at the mouth of the Mississippi River and the formation of the hypoxic zone. This information is not presented in the current report.

7. *Significance of Data From USGS Monitoring Stations Below St. Francisville?*

Reviewer 1

Information from USGS monitoring stations below St. Fancisville are important to evaluating nutrient loads in the Mississippi River and northern Gulf, particularly in light of the fact that they exclude the major impacts of metropolitan areas down river and gulf side of this sampling station. Although there will always be other reports that show conflicting results and interpretations as regards to the source, seasonality and loadings of the northern Gulf, we have to gleen information from each report to make the important decisions needed. Whatever the conflicting conclusions from other reports (such as "Nutrient Releases to the Mississippi River in the Louisiana Industrial Corridor: Voluntary Reductions in Nitrogenous and Phosphatic Compounds"), the main premise is still valid; there must be a holistic and sustainable approach to nutrient management in the Mississippi and Alchafalaya River Basins, such that loads of both P and N are decreased. Only then will there be a lasting and widespread decrease in the severity and extent of hypoxia ion the northern Gulf.

Reviewer 2

No answer provided.

Reviewer 3

The Region 4 reports (January and April drafts only) make the point that nutrient loadings to Gulf may be underestimated if derived from St. Francisville (river mile 266, above Baton Rouge) data because of downriver point-source discharges and attribute the higher concentrations and estimated loadings for P and DIP at Belle Chasse (river mile 76, below New Orleans) to those additional loadings. To assess the quantitative significance of the lower river discharges I assembled loading estimates for the Lower Mississippi River (LMR) below the Old River Control Structure (thus excluding the Atchafalaya River discharge) (Table 2). Included in this table are the loadings reported in both the CENR integrated assessment (Goolsby et al., 1999) and the Region 4 reports and the LMR point source additions derived from the release inventories assembled by Knecht (2000).

For forms of DIN (nitrate, nitrate and ammonium), the results are in close agreement. LMR point-source releases (mainly treated sewage discharges) are less than 1% of the upriver loadings and Region 4's Belle Chasse loading calculation is actually lower that that for St. Francisville.

While the CENR and Region 4 loading estimates for total P and DIP (phosphate-P) at St. Francisville are in very good agreement, the loadings estimated by Region 4 at Belle Chasse are 42 and 34% higher than at St. Francisville for TP and DIP, respectively. The first question one should ask concerns the comparability of the Belle Chasse data. It stretches the imagination that 42 million kg of total P were added to the LMR from point sources, given the tremendous fluxes of particle-borne P moving down the river. I have heard concerns from USGS scientists about the reliability of the Belle Chasse P data and data comparability should be checked. However, an 11 million kg increase in DIP is consistent with the reported 15 million kg loading of phosphoric acid (dissolved phosphate) from point sources, primarily runoff from phosphogypsum piles at fertilizer plants, especially after assuming that some of dissolved P adsorbs onto suspended sediments in the river.

Table 2. Comparison of annual average loadings of forms of nitrogen (N) and phosphorus (P) for the Lower Mississippi River (LMR) below the Old River Control Structure

Property	Loading 10^6 kg yr	Period	Location	Source
Nitrogen				
Total N	1182	1980-96	St. Francisville	Goolsby et al. 1999
Nitrate-N	732	1980-96	St. Francisville	Goolsby et al. 1999
Nitrate+nitrite-N	713	1980-99	St. Francisville	EPA R4 (Jan., Apr.)
Nitrate+nitrite-N	703	1980-99	Belle Chasse	EPA R4
Ammonium-N	23	1980-96	St. Francisville	Goolsby et al. 1999
Ammonium-N	34	1980-96	St. Francisville	EPA R4 (Jan., Apr.)
Ammonium-N	34	1980-99	Belle Chasse	EPA R4
Nitrate+ammonium-N	738	1980-96	St. Francisville	Goolsby et al. 1999
Nitrate+nitrite-ammon.-N	748	1980-99	St. Francisville	EPA R4 (Jan., Apr.)
Nitrate+nitrite-ammon.-N	736	1980-99	Belle Chasse	EPA R4
Nitrate + ammonium-N	7	1987-93	LMR point sources	Knecht 2000
Nitrate + ammonium-N	7	1994-98	LMR point sources	Knecht 2000
DON	286	1980-96	St. Francisville	Goolsby et al. 1999
PON	153	1980-96	St. Francisville	Goolsby et al. 1999
Phosphorus				
Total P	97	1980-96	St. Francisville	Goolsby et al. 1999
Total P	100	1980-99	St. Francisville	EPA R4 (Jan., Apr.)
Total P	142	1980-99	Belle Chasse	EPA R4 (Jan., Apr.)
Orthophosphate-P	31	1980-96	St. Francisville	Goolsby et al. 1999
Orthophosphate-P	31	1980-99	St. Francisville	EPA R4 (Jan., Apr.)
Orthophosphate-P	42	1980-99	Belle Chasse	EPA R4
Phosphate-P	15	1987-93	LMR point sources	Knecht 2000
Phosphate-P	5	1994-98	LMR point sources	Knecht 2000

Note: there is a discrepancy between the stated annual average transport of 35,400 metric tons (EPA R4, January, p. 16) and the load expressed as 85 metric tons/day ion Table 3. The latter appears to be correct (see Figure 8).

If the point source loading estimates are reasonably accurate and all of this P is reactive (bioavailable) upon reaching the Gulf, as much of 19% of the reactive P discharged by the LMR could have been from these point sources during the 1980s through 1993. This is based on the assumption that all of the DIP and 48% of the particulate P (see 8, below) flowing past St. Francisville comprise the remainder of the reactive P load. However, with the reported reductions in industrial releases of phosphate (Knecht, 2000), these point sources would be expected to comprise only 7% of the reactive P load in the late 1990s (based on the 1994-98 average). If this is accurate and the upriver sources are held constant, loadings of reactive P down the LMR (excluding the Atchafalaya) should have abruptly declined by 13% after 1993.

Because of the potential significance of LMR phosphorus point-source loading, I agree with the recommendations of the Region 4 reports that both additional mining and analysis of LMR monitoring data and reestablishment of regular monitoring below New Orleans are warranted. In that regard, I found it curious that the Region 4 reports (January and April drafts) present only nitrate data from the Luling USGS station. Assuming P data are available for this site, flux estimates for TP and DIP might be useful in validating results derived from the Belle Chasse site.

Reviewer 4
Region 4 report has little or no significance. It presented no systematic comparison of USGS monitoring at St. Francisville vs locations further down stream. Knecht (2000) indicate that there was little difference in nitrate concentrations between St. Francisville and Belle Chase 1988-1993. On the other hand there were much higher P concentrations at Belle Chase than St. Francisville 1988-1990. This difference was eliminated 1991-1993, and the hypoxic area increased substantially starting in 1993. This may be evidence against a significant role for P in hypoxia, but the Knecht study reports total P in the river, not inorganic P, which may be the most relevant form of P to productivity and hypoxia. In terms of monitoring, there is a need for monitoring further downstream, which has been recognized in “A Science Strategy to Support Management Decisions Related to Hypoxia in the Northern Gulf of Mexico and Excess Nutrients in the Mississippi River Basin” (The Mississippi River/Gulf of Mexico Watershed Nutrient Task Force, 2004).

Reviewer 5
No answer provided.

Reviewer 6
The USGS monitoring stations below St. Francisville (primarily Belle Chasse) are crucial to evaluating the nutrient loads to from the Mississippi River to the Gulf. It was a major mistake to partially deactivate this station in the early 1990s and it should be reactivated a soon as possible. The recent decrease in P discharges in the Mississippi River Industrial Corridor and apparent declines in P at Belle Chasse after 1990 additionally argue for the importance of the Belle Chase station (Knecht, 2000). In the long run the N and P levels at Belle Chase may converge with those at St. Francisville, as they did in the early 1990s. However, until that trend is clearly established, the Belle Chasse station remains important. Additionally, as the station on the Mississippi River closest to the Gulf, it should be continued in its own right.

Reviewer 7
The Region 4 report is fatally flawed in its reliance on consideration of only dissolved inorganic phosphorus. It is of no value in evaluating further study.

Reviewer 8

The Region 4 documents show no significant differences in DIN concentrations between St. Francisville and Belle Chase USGS monitoring stations for the period of 1980-1999. However, there is a systematically higher mean DIP concentration at Belle Chase, suggesting possible point source inputs. The Knecht (2000) report provides evidence that point sources of phosphoric acid along the lower river have been reduced since 1994. From my perspective, I do not see that this substantially changes the overall findings of the Region 4 reports, other than to make phosphorus limitation even more likely. The other conclusion I draw from these results is that nutrient monitoring along the lower river is critical.

Reviewer 9

The Knecht 2000 report indicates that voluntary measures decreased P release from fertilizer plants from about 180,000,000 to 20,000,000 lbs/ year of phosphoric acid. I assume this does not mean phosphoric acid P, but the weight of the H_3PO_4 since this point is not clarified in the Knecht report. This reduction of phosphoric acid loading rate is about 160,000,000 lbs/y, which can be converted to 7.26×10^{10} g/y. At 98 grams per mole of phosphoric acid this leads to a reduction in loading of 7.41×10^8 mol/y or 2.03×10^6 mol P/ d. The region 4 report estimates SRP loading from the Mississippi (Belle Chasse) and the Atchafalaya River (Morgan City) at 114×10^6 and 28×10^6 metric tons per day, respectively. This sums to 1.42×10^8 g SRP-P per day or 4.6×10^6 mole SRP-P per day. While this is a very rough calculation, it means around 44% of the SRP load could have been reduced by voluntary reductions. However, total P load is more relevant. Given the ratio of total P to SRP for Belle Chasse (2.33 calculated from table 1 of the region 4 report), the reduction could account for approximately 19% of the total P load. The Knecht 2000 report indicates sharp decreases in SRP loading in 1989 and 1994, and these do apparently correspond with times of decreasing SRP concentrations at Belle Chasse (Figure 3 of the Region 4 report).

These data suggest that Belle Chasse is an extremely important USGS station below St. Francisville because some human impacts may occur below this point, although the impact of the Baton Rouge and New Orleans sewage inputs are relatively minor.

8. *The Redfield Ratio as used by Rabalais et al. (1999) in the Mississippi River has Been Called Into Questions. Please Comment on the Use of the Redfield Ratio in the Region 4 Report Evaluation, and the Use of this Calculation to Identify the Limiting Nutrient in the Water Column of the Mississippi River and the Northern Gulf of Mexico*.

Reviewer 1
No answer provided.

Reviewer 2
No answer provided.

Reviewer 3
The single-minded focus on the DIN:DIP ratio in the Region 4 report overextends the reliability of this ratio for predicting nutrient limitation and misrepresents the loading of bioavailable phosphorus to the northern Gulf, thereby unfairly assailing the use of DIN:TP by Rabalais et al. (1999).

a. **Misuse of DIN:DIP**. In a paper entitled “Misuse of organic N and soluble reactive P concentrations to indicate nutrient status of surface waters,” Dobbs (2003) calls into question the utility of the ratio of DIN:SRP (soluble reactive phosphorus) to indicate nutrient deficiency. He cites as reasons the well-known problems associated with the determination of SRP and the fact that concentration values are in units of mass per unit volume and cannot be used with certainty to estimate nutrient supply (expressed in mass per unit volume per unit time) without information on uptake and remineralization. In other words, it is not the concentration of a nutrient that limits production, but its supply rate. Based on continental-scale data sets from flowing surface waters, Dodds concluded that if DIN:SRP is extremely high (e.g. 100:1), then N deficiency is unlikely because DIN is not in short supply. Likewise, if DIN:SRP <1 then it is unlikely that P is limiting. At the intermediate levels, which include all of those for the river and most of those for the Gulf as reported in the Region 4 report, there might be no nutrient limitation whatsoever, because both DIN and SRP could be very high and something else (e.g., light) is limiting production, or there might be limitation by one or the other of these nutrients. The Region 4 reports present a classic case of “misuse” from the Dobbs perspective. It treats the Redfield ratio (16) for DIN:DIP as if it were a precise tipping point between N or P limitation, and it is not.

b. **Underestimation of bioavailable P**. Using the DIN:DIP ratio to determine the nutrient limiting phytoplankton growth in the river itself is rather meaningless, because growth is usually light limited, except under low-flow conditions. In using DIN:DIP in the river discharge to predict nutrient limitation on the shelf, the Region 4 report assumes that particulate P delivered by the river to the Gulf is not available to support phytoplankton production. However, it has long been known that reactive P adsorbed to particles in fresh water is released as salinity increases (Lebo, 1990) and that iron or sulfate reduction in marine sediments can remobilize and release reactive P (Caraco et al., 1990). In a recent paper specifically addressing the flux of reactive P from the lower Mississippi to the Gulf of Mexico, Sutula et al. (2004) concluded that P released from sediments is likely to play a major role in supporting water column productivity on the shelf for three reasons: (1) a five-fold greater release of P from high-sulfate marine sediments than in freshwater systems; (2) highly energetic conditions on the inner shelf (waves, shrimp trawls, etc.) that cause physical mixing and reworking of sediments that speed up diagenic processes; and (3) P buffering in the estuarine mixing zone that causes substantial desorption of P from suspended sediments.

Sutula et al. estimate that 98% of the suspended particulate TP and 92% of the TP in sediments at the bottom of the river potentially contribute to the ocean's reactive P pool ultimately; although 58% and 31%, respectively, are more conservative estimates relevant to reactive P release during residence on the continental shelf. Using this more conservative estimator they calculated an average riverine export of reactive P of 87 x 10^6 kg yr^{-1}, in contrast to the Region 4 estimate of 42 x 10^6 kg yr^{-1} based on DIP alone, i.e. more than twice the loading. This means that the effective DIN:RP ratios, a better indicator of bioavailable loadings, would be less than half the DIN:DIP ratios reported by Region 4 and more closely approximating the Redfield 16:1 stoichiometry. Finally Sutula et al. recognized that this loading calculation underestimates total reactive P flux, because it is based on surface water TP flux alone. Remobilization of channel sediments and deposition to the Gulf of Mexico shelf during pulsing events is also likely to play an important role in the P budget of the Gulf shelf.

With this understanding, the pointed criticism of the use of nutrient loading ratios based on TP by Rabalais and co-workers and the CENR assessment is off-base. While the Region 4 report (January, April drafts) states that the authors could find "no other instance where total phosphorus was used for the computation of Redfield ratios," in fact this is quite common as a screening tool to determine potential nutrient limitation for coastal waters. For example, the National Research Council's (2000) treatise on coastal nutrient pollution includes a figure (Figure 3-4) comparing N:P ratios for 28 ecosystems.

Reviewer 4

Rabalais et al. (1999) identify reasons why the Redfield ratio can be a misleading indicator of the limiting nutrient. In spite of the different use of the Redfield ratio in the two studies, I see little difference in the conclusions of the Region 4 report and Rabalias et al. (1999) relative to the occurrence of P limitations in the Northern Gulf of Mexico.

Reviewer 5

No answer provided.

Reviewer 6

The Redfield ratio as used by Rabalais et al. (1999) is unusual since in the river water it refers to DIN:TP, while in the Gulf it refers to DIN:DIP, the more conventional usage. I am not sure why the river data was treated this way, perhaps due to data availability, but the EPA report is correct to point out the unusual application of this ratio and the possibility of confusion. Nonetheless, nutrient ratios alone are only suggestive of nutrient limitation (see Questions 1 and 2), and the nutrient concentrations and ratios listed in Dortch and Whitledge (1992), as referred to in the report, are probably a better starting point for predicting nutrient limitation than the Redfield ratio. (Dortch and Whitledge, 1992: P limitation possible at P less that 0.2 uM and N:P > 30, N limitation at N less than 1 uM, and N:P <10, N is DIN, P is DIP). Geider and LaRoche (2002) have determined similar criteria based on culture studies.

Reviewer 7

I am a firm believer in the Redfield ratio concept, but it must apply to the bioavailability of nutrients in the receiving water. The Region 4 report is completely wrong in their use of this concept here.

Reviewer 8
No answer provided.

Reviewer 9
The Redfield ratio is not used correctly in either report, but the use by Rabalais et al. (1999) is closer to correct. It should be noted that Redfield data are not the only data used by Rabalais et al (1999) to indicate nutrient limitation. The region 4 report evaluation suggests that they calculate the Redfield ratio in the traditional way, and this is superior to the Rabalais et al. (1999) application. Redfield’s logic was different and was based on a steady-state argument. He suggested that algae grown under nutrient replete conditions had a composition similar to what we now call the Redfield ratio. Then he argued that the reason inorganic nutrient concentrations in the ocean are at the same concentration as in cells is that the organisms control that concentration.

More recent research has demonstrated that Redfield ratios using dissolved inorganic nutrients are poor indicators of nutrient limitation. For example, Dodds et al. (1997) demonstrated that inorganic nutrient concentrations in the water column of rivers and streams were poor predictors of the benthic algal biomass, but that total nitrogen and phosphorus concentrations were significantly better.

The research of Fisher et al. (1992) evaluated growth based bioassays of nutrient concentrations as a function of dissolved inorganic nutrient concentrations in the Chesapeake Bay. During May 1987, when DIN/SRP values were mostly above 100, there was no response or an exclusive P response. However, when this ratio was close to 1, there was primarily a P response and secondarily an N response. In August 1987, there was a primary N response and secondary P response when the DIN/ SRP ratio was 10, 1, and 2, and an exclusive N response when DIN/ SRP was 1 and 0.8. This suggests that the annual and spring DIN/SRP ratios (40-50) reported for the Mississippi River in the Region 4 report do not indicate clear exclusive P limitation, but the potential for limitation by N or P in the region most influenced by nutrient runoff from the Mississippi basin.

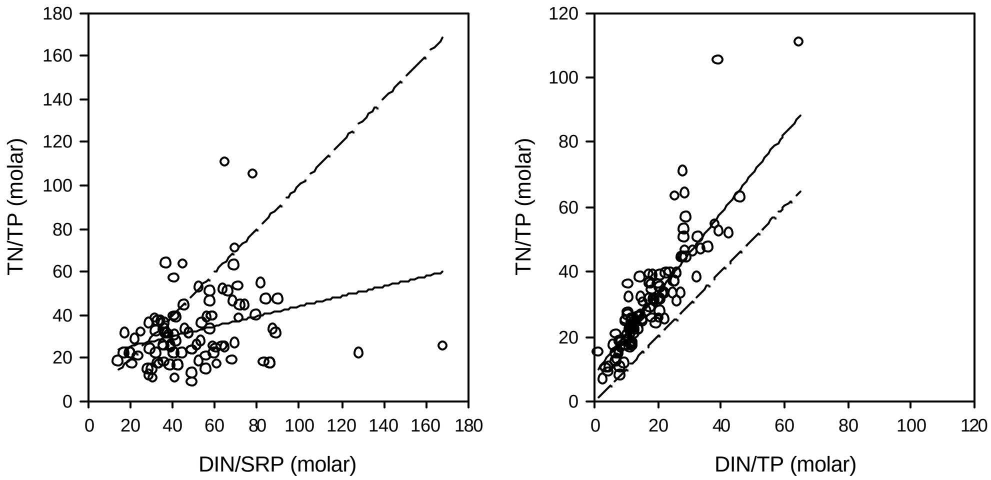
A better estimate of limitation would come from the loading of total N and total P to the Northern Gulf, and then adjusting for differential losses of the two elements. Such losses might include sedimentation of adsorbed P and denitrification of nitrate. The data available for the Mississippi and the Atchafalaya suggest a poor correspondence between DIN/SRP and TN/TP ratios (Figure 1, Table 1). DIN/TP ratios as used by Rabalais et al. (1999) provide a better estimate (Figure 1, Table 1) but an estimate that is too high by adding about 8.6 to values (e.g. the slope is about equal to 1, but the intercept is 8.6). This error would somewhat underestimate the degree of N loading leading to a potential overestimation of the degree of N limitation. The situation is more complex for DIN/SRP ratios, however at the mean ratios reported for the Belle Chasse and Morgan City stations, the DIN/SRP ratios are expected to underestimate TN/TP and lead to an underestimation of the degree of N limitation. The variance between DIN/SRP as related to TN/TP is much greater than DIN/TP as related to TN/TP.

Table 1. Statistical relationships between nutrient ratios at the sites of interest. See Alexander et al. (1998) for description of data sources.

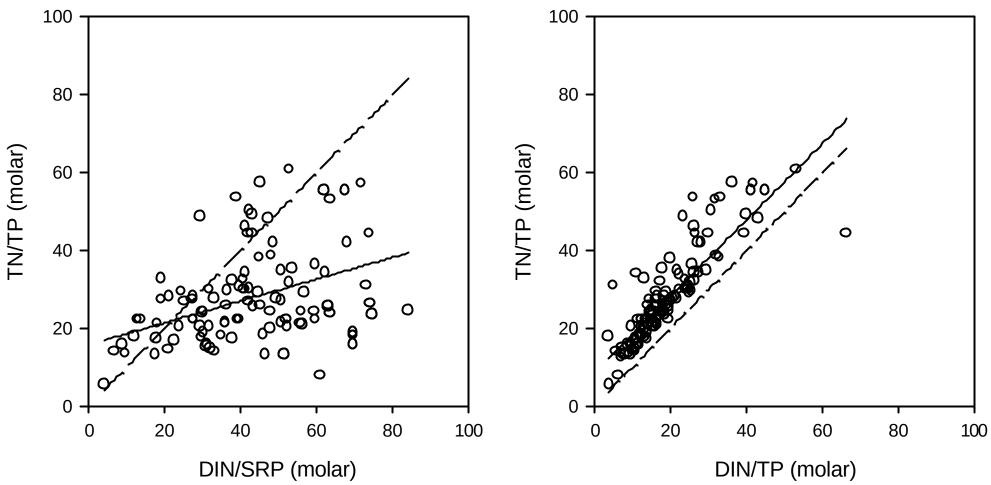
Site	Dependent	Independent	Intercept	Slope	R^2	P	N
Mississippi River at Belle Chasse, LA	TN/TP	DIN/SRP	15.79	0.28	0.13	<0.003	116
Mississippi River at Belle Chasse, LA	TN/TP	DIN/TP	8.63	0.98	0.81	<0.000	139
Lower Atchafalaya	TN/TP	DIN/SRP	21.0	0.231	0.09	.0028	97
Lower Atchafalaya	TN/TP	DIN/TP	8.74	1.23	0.77	<0.000	115

Figure 1. Relationships between the nutrient ratios at the two stations of interest. All USGS data were taken for the two stations in the region 4 report and are described by Alexander et al. (1998). Only sampling events were included where data for DIN, SRP, TN and TP were all available. Dashed line represents the 1:1 relationship, and the solid line the relationship described with the equations in Table 1.

LOWER ATCHAFALAYA R @ MORGAN CITY, LA



MISSISSIPPI RIVER AT BELLE CHASSE, LA.



9. *Please Identify Gaps in the Data and Information, and Provide Additional References and Data Resources Where Possible*

Reviewer 1
No answer provided..

Reviewer 2
No answer provided.

Reviewer 3
The following research needs identified in the January and April drafts of the Region 4 report are red herrings and should not, in my opinion, be considered priorities:

a. Light limitation. Light limitation is of course an important factor affecting primary production, but it is reasonably well understood. Although soil conservation practices (including conservation tillage) have contributed to reduced sediment loading, the principal cause of reduced suspended sediment loads in the lower river has been dam trapping. Most of this load reduction occurred before the exacerbation of hypoxia in the mid-1970s.

b. Carbon impacts to hypoxia. This was examined in the CENR integrated assessment which developed a scientific consensus that direct discharge of organic carbon from the rivers is a relatively small factor in hypoxia (Rabalais, et al., 2002).

c. Role of nitrogen fixing by cyanobacteria in the Gulf. The Region 4 report (January and April drafts) refers to literature (e.g. Tyrrell, 1999) that suggests that nitrogen fixation is efficient enough to ensure that marine waters are never nitrogen limited when there is an adequate supply of available iron. However, the NRC (2000) refuted this, pointing out that, although this may be so for open ocean waters, "nitrogen fixation simply does not occur in most estuaries and coastal seas and does not alleviate nitrogen shortages." For a variety of reasons discussed in that report, nitrogen fixation by cyanobacteria tends to be insignificant in temperate coastal waters, except in low salinity conditions with ample reactive P supplies but insufficient dissolved N. The phytoplankton of the Louisiana shelf has been well studied and there is no observational evidence that cyanobacteria are important there.

The remaining two research needs, nutrient bioavailability and denitrification, are important, but I would reframe them in suggesting that the most critical research priority with regard to determining whether P source reductions would be effective in reducing hypoxia is to quantitatively understand nutrient (both P and N) recycling and primary production in a context that relates to hypoxia formation and maintenance. A rare glimmer of insight shines through in Section 5.2.3 of the January and April drafts of the Region 4 report where it is stated: "In shallow systems like the Mississippi River plume and Northern Gulf of Mexico it is highly artificial to consider nutrient concentration and supply ratios based only on what is found in the water column at any point in time and space. N and P regeneration rates need to be established and incorporated into the overall nutrient cycling and availability schemes." Why this good advice is ignored in the overzealous focus on initial nutrient concentration ratios is mystifying (see 10 b).

Reviewer 4

Rather than making the case for P limitation, which has already been accepted, it might be more useful to have some quantitative estimation of the portion of the productivity or hypoxia in the northern Gulf that is controlled by P limitations. Recognition of the limitations of Redfield ratio for identifying the limiting nutrient would be helpful. Finally, if P riverine P delivery to the Gulf has a significant control on hypoxia, it would be instructive to explanation why the area of hypoxia increased significantly after the large reduction in point source P inputs to the Mississippi River in Louisiana starting in 1991.
Information contained in the following document would probably be useful:

National Research Council. 2000. Clean Coastal Waters: Understanding and Reducing the Effects of Nutrient Pollution. National Academy Press, Washington DC.

Reviewer 5

No answer provided.

Reviewer 6

This report is a useful contribution, especially as a summary of the river data, but lacks important information in many areas. The earlier versions had more river data, some of which was useful, though their major case can be made from what remains. I found the river section of the earlier documents more useful than the current one. The limited review of gulf data in the discussion section is intended to support the arguments made from the river data, but leaves out a lot of information. I have suggested specific corrections and additional references below.

Reviewer 7

I have identified a few of the key references, below. A starting point would be a careful reading of the report of the National Academy of Sciences (NRC 2000), which apparently has not caught the attention of the staff who wrote the Region 4 report. Beyond that, there is a great abundance of appropriate literature and information. But my reading of the 3 draft EPA region 4 reports leads me to conclude that the analysis should not be left to the same staff in that region. They appear to lack the objectivity, the energy to look at the literature of the last 25 years, and/or the ability to contribute meaningfully to this debate.

Reviewer 8

No answer provided.

Reviewer 9

The best way to indicate the degree of N or P deficiency is with properly executed bioassays. Preferably these bioassays would be conducted as outlined by Fisher et al. (1992), except the incubations would be long enough to allow growth (increase in chlorophyll) to occur. Secondary data that can be used to indicate deficiency are particulate C:N and C:P ratios (as used by Dodds and Priscu 1990). This physiological technique should be applied from the initiation of the zone of the Gulf where non-algal turbidity has mostly dropped across a transect, to the low nutrient, high salinity waters outside the major zone of influence of the Mississippi River plume. The work by Smith and Hitchock (1994) needs to be repeated with more extensive temporal and spatial coverage.

V. SPECIFIC COMMENTS

Page ii
Reviewer 8
Abstract – the statement “Phosphorus appears to be the limiting factor in areas of the Gulf where phytoplankton growth is greatest, especially during the critical late winter and spring growing season” is too broad too be supported. Yes, there is evidence that phosphorus limitation may be important, particularly during spring and in lower salinity regions. Yes, this coincides with regions of high algal productivity (not necessarily “where phytoplankton growth is greatest”). The evidence that processes during late winter and spring are “critical” is less compelling, and highlights one of the key areas of uncertainty. That is, where and when production of organic matter fueling hypoxia occurs is not yet clear. However, the fact of the matter is that it DOES occur, while understanding where and when will enhance our ability to regulate it, *this does not mean that good management practices should be delayed until it is determined if production of organic matter fueling hypoxia occurs mainly in spring, or in fact over a longer period including spring and summer.*

Pages 5, 6 and 9
Reviewer 6
Tables 1, 2, and 3 need “N” values, the number of measurements in each category. Similar information would be useful for Figs. 6 and 7.

Page 9
Reviewer 8
The January 2004 document provided a historical overview leading to the EPA Region 4 review of the CENR reports and the Hypoxia Action Plan. A brief summary of these documents was also provided on page 7. The CENR reports should be cited consistently by author and year, rather than as “CENR Reports 1-6.” It was stated that the document focused on the CENR Reports 1 (Rabalais et al., 1999), 3 (Goolsby et al., 1999), and 4 (Brezonik et al., 1999). The very next sentence was contradictory stating, “review of CENR Reports, 2, 4 and 6 were deemed to be unnecessary.”

Page 9
Reviewer 8
The EPA January 2004 review was particularly critical of CENR Report 1 (Rabalais et al., 1999). It was argued that this report presented conflicting views for nitrogen limitation on the one hand and near Redfield ratio nutrient levels on the other. In fact, this document presents evidence for limitation by both phosphorus and nitrogen. On page 114, “Nitrogen is the limiting nutrient for overall productivity in the northern Gulf of Mexico (Table 6.6), but other nutrients at times can occasionally become limiting.” More importantly, the Rabalais et al. (1999) report points out that nutrient levels are far in excess of what is considered saturating for phytoplankton uptake over large areas of the northern Gulf of Mexico in the vicinity of river outflow. The Rabalais et al. (1999) report does argue that nitrogen is the limiting nutrient for overall productivity, and I would submit that this is inference and not entirely supported by the data presented.

The January 2004 document contended that “Reports 4-6 address only nitrogen control.” This is incorrect. Report 4 (Brezonik et al., 1999) modeled effects of both N and P reductions, and found that both nutrients had the potential to reduce the severity of hypoxia. To quote from p. 199, “Dissolved oxygen and chlorophyll concentrations on the Louisiana Inner Shelf (LIS) appear responsive to reductions in N and P loadings from the Mississippi–Atchafalaya River (MAR)." Although differences in results between N- and P-loading reductions generally were not large, there was a tendency for responses to be somewhat greater for N-loading reductions than P-loading reductions, especially for dissolved oxygen under reduced boundary conditions." *Even more importantly, the Brezonik et al. (1999) report aptly acknowledges key areas of uncertainty in modeling efforts where better information is needed.*

Page 10
Reviewer 8
The document discusses the concept of Redfield ratios as a criterion for stoichiometric nutrient balance and suggests that deviations from the Redfield ratio of 16:1 DIN:DIP can provide an indication of whether N or P are limiting. While this is partially true, another key criterion is whether the concentration of the nutrients are below thresholds for saturation of algal nutrient uptake. This latter point seems to be neglected in many cases, yet it is critical. Another related issue is ambient concentrations of nutrients do not necessarily reflect their rate of supply. Quoting from an August 2004 letter written by Donald F. Boesch (University of Maryland) to Benjamin H. Grumbles, Acting Assistant Administrator for Water, Environmental Protection Agency: "Ratios of inorganic N to P are often not good indicators of the relative availabilities of N and P in coastal systems because of the differing biogeochemistry of these elements (Howarth and Marino, 1990. Limnology and Oceanography 35: 1859-1863). This is particularly so when the inorganic N:P ratio is within a factor of 2-3 of the Redfield ratio of 16:1 by moles. True determination of nutrient limitation depends upon knowing the *rates of supply* of N and P, which are affected by the faster recycling of organic P than N, N losses due to denitrification, and exchanges of P with both bottom and suspended sediments. This is discussed in detail in the National Research Council's 2000 report, *Clean Coastal Waters: Understanding and Reducing the Effects of Nutrient Pollution*…" This issue is further complicated by the fact that algal biochemical ratios of N:P, and ratios of uptake, can change depending on nutrient state (Geider and LaRoche, 2002) and differ substantially among taxa (Quigg et al., 2003). Thus, conditions that specify limiting nutrients may differ among algal assemblages depending on physiology and species composition. Nonetheless, numerous types of evidence support the potential for both phosphorus and nitrogen limitation.

Page 12
Reviewer 8
The January 2004 document discussed the FLUX program for calculating loadings, but never addressed this method in comparison to the multiple regression method of Goolsby et al. (1999). Do they differ in systematic ways?

Page 12, Paragraph 1
Reviewer 8
The reference nutrient concentrations are even lower than the EPA recommended values (Smith et al. 2003).

Page 12, Table 4, Legend
Reviewer 8
Is the hypoxia characterization report the Rabalais et al. 1999 report?

Page 12, Table 4

Reviewer 8
The terminology bounces between molar and mass/ volume units.

Page 13
Reviewer 4
References to Figures 6.5 and 6.6 in Rabalais et al. 1999 seem to be incorrectly switched around. That is, they refer to Figure 6.5 when I think they mean 6.6 and vice versa.

Page 13, First Paragraph
Reviewer 6
Cites 3 figures from Rabalais et al., 1999, Figs. 6.5, 6.6, 6.7. Fig. 6.6 is referred to as showing nutrient gradients, when in fact it shows phytoplankton pigments, as does Fig. 6.7. Only Fig. 6.5 shows nutrient gradients.

Page 13
Reviewer 8
It was unclear why detailed river discharge data were given. The authors’ objectives should be made clear. This seems unnecessary. The data were referenced as from Winstanley et al. (2003), but I believe the actual source is the Army Corps of Engineers.

Page 15, Last Paragraph
Reviewer 4
The text refers to Figure 8, and probably should be Figure 9.

Page 15, Last Paragraph
Reviewer 6
Cites DIN:DIP ratios in Fig. 8 when it means Fig. 9.

Page 15
Reviewer 8
I think Figure 9 is what is referred to here.

Page 16, Second Paragraph
Reviewer 6
Refers to chlorophyll concentrations along transect C. It refers to Fig 9 when it should be Figure 10.

Page 16, Figure 9
Reviewer 8
Add error bars?

Page 16, Last Paragraph
Reviewer 4
The reference to Figure 9 in the text probably should be Figure 10.

Page 16, Figure 10
Reviewer 4
According to the text, this figure illustrates the highest productivity occurring in the spring and early

summer, but the timing of each column of data is not clear. Does each column represent a month? If so, the gap between 1/1/1995 and the first column of data and then gaps at the end make it difficult to determine what month each column represents that year. This seems clearer in 1997, but the highest productivity.

Page 16

Reviewer 8

The statement is made that "mass transport of phosphate shows less month to month variation than DIN." I would disagree. Proportionately, these are very similar. Concentrations of DIP are less variable.

Page 21, Table 3

Reviewer 8

Is not referenced in the text.

Page 24

Reviewer 8

The document cites Correll (1998) as stating that "Orthophosphate is the principal form of dissolved P and the only form of P that can be utilized by algae." This is too broad of a statement. Organic forms phosphorus may be at least partially available to autotrophs. In fact, determining the availability of dissolved P is a challenging problem (Hudson et al., 2000), and certainly not as clear-cut as this statement would lead one to believe. On this same page, the document is critical of the interchangeable use of DIN:DIP and DIN:TP ratios to infer relationships to Redfield ratios. Clearly, for such determinations the use of DIN:DIP is more conventional, and the document makes a valid criticism of the somewhat arbitrary use of the DIN:TP ratio.

Page 27, Figure 22

Reviewer 8

Is redundant.

Page 30, Bottom

Reviewer 8

While it is true that recent data do not demonstrate N limitation exclusively, it is also not true that there is exclusive evidence for P limitation. Both may occur at different times and regions, and both nutrients are present in excess over much of the region.

Page 32

Reviewer 8

The January 2004 document cites the Rabalais et al. (1999) report as stating that organic matter produced during the spring and early summer is a major cause of hypoxia. In fact, the report notes that data showing organic matter fluxes are limited, and that the actual timing in supply of organic matter fueling oxygen consumption is not well known.

Page 35

Reviewer 8

There are various Navy hydrodynamic models, some of which may not be of sufficient spatial resolution to address the problems mentioned. This statement is unclear.

Pages 37-38, Conclusions

Reviewer 8

The January 2004 document seems to discount much of the discussion of both N and P in the CENR reports. Furthermore, it concludes that primary production of organic matter during spring is the primary source fueling hypoxia, but this is not well supported by the data. While the January 2004 document raises some valid questions, it goes too far in its conclusions and misquotes the CENR reports in numerous instances.

VI. MISCELLANEOUS COMMENTS

Reviewer 1
Numerous specific comments of an editorial nature have been made directly on the document. These detract from the scientific impact of this report and do not do the authors justice in light of the obvious large amount of work, time and effort that has been put into this study and its reporting.

Reviewer 6
The word phosphorus is spelled both as "phosphorus" and "phosphorous". Both are valid, but the first is more common and the usage should be consistent.

Reviewer 6
I am not sure of the point of this report, but if it is to provide a review of the evidence for potential P limitation in the gulf, and therefore the need to consider P as well as N control, it needs substantial revision. All the evidence for P limitation, including the gulf as well as the river data, should be given in the results section. The discussion section should be reserved for discussing the relationship of the data to the literature, as well as the interpretations, conclusions, and implications. In addition, additional literature citations are needed (see below).

Reviewer 6
I agree with this report that in general, control of <u>both</u> N and P needs to be considered in this system, as well as in many similar estuarine and river-impacted coastal systems (Conley, 1999; Mee, 2001; Paerl et al., 2004). The earlier versions of this report did not argue for the continued importance of N control.

Reviewer 6
However, there remain many unknowns, some of which are referred to briefly in this report. These include the role of density stratification, the supply of nutrients (especially P) from the sediments, the relationship between primary productivity and particle flux and hypoxia—especially in space and time, and others. The issue of DIP supply in marine waters from the total P in inflowing rivers is also not addressed. This is an important source of DIP to coastal regions since some of the P found on particulate P in rivers (the dominant P component in rivers), comes off as DIP when it hits salt water (see the L.E. Fox references below). There is a lot of ongoing research on some of these issues and such a report will need to be continually updated.

Reviewer 8
Provide more detail on the loading calculations.

Reviewer 9
Total N and total P loading estimates should be reported where available, as should more recent nutrient chemistry values.

Reviewer 9
If more recent data are available they should be reported. Perhaps working with the USGS would help here.

VII. COMMENTS REGARDING: "REVIEW OF ISSUES RELATED TO GULF OF MEXICO HYPOXIA," APRIL, 2004

Reviewer 8

This document differed only slightly from the January 2004 document. Conclusions of the two documents were virtually identical. The primary differences evident to this reviewer were that the abstract was substantially shortened relative to the January 2004 document, the redundant figure was omitted, and a better quality map of the EPA monitoring stations was provided (Fig. 20), showing the location of Transect C stations. A legend was added to Fig. 7. Various other edits were made which did not substantially alter the content from the January 2004 version. Therefore, no further review is given here.

VIII. COMMENTS REGARDING: REVIEW OF "EVALUATION OF THE ROLE OF NITROGEN AND PHOSPHORUS IN CAUSING OR CONTRIBUTING TO HYPOXIA IN THE NORTHERN GULF," AUGUST, 2004

Reviewer 8

This document is substantially abbreviated in comparison to earlier documents. In addition, the title was changed. Information in this latest version focuses mainly on nutrient ratios, and implications for limitation of primary production. Unfortunately, this document has been so truncated as to diminish its efficacy. The primary conclusion, that phosphorus needs to be considered in nutrient reduction strategies, is consistent with the earlier versions. However, much of the supporting data were omitted, leaving the reader with a less convincing body of evidence.

Specific comments:

Title

Reviewer 8

The title of this document is misleading as it does not even address hypoxia. The August 2004 document does a cursory job of dealing with the nutrient limitation issue, but even in this case, much of the supporting information in the earlier versions was omitted.

Page 1

Reviewer 8

The Brezonik et al. (1999) was not in the references. The statement near the bottom of the page that DIP is the only form utilized by autotrophs is not accurate.

Page 2-4, Approach

Reviewer 8

More detail should be given regarding methods. This should include an explanation of what is meant by "grab-sample nutrient concentrations". Also nutrient detection limits should be given, and how data were processed. Nutrient values at or below detection should be discarded from calculations of ratios as this could result in anomalous estimates.

Page 5, Figure 2

Reviewer 8

In this version of the document, individual nutrient values are provided in contrast to averages presented in earlier drafts. I think both presentations have merit. Does the FLUX program provide a concentration value mapped to river flow? If so, this might be useful to overlay on the individual nutrient values, to give an indication of how representative the estimated concentrations are.

Pages 6 and 7, Figures 3 and 4

Reviewer 8

Same comments as for Figure 2 above.

Page 13
Reviewer 8
The statement "This trend positively correlates with the nutrient concentration gradients presented…" is not supported by the information given in either the August 2004 document or the cited Rabalais et al. (1999) CERN report. There appears to be a relationship, but it was not demonstrated statistically. Also, it was unclear what was meant by the statement "the reactive ortho-phosphate concentrations appear to be relatively constant in the Hypoxic Zone downstream of the initial algal production zone…" Downstream of what? And what is the "initial algal production zone"? This section was difficult to follow.

Page 14, Paragraph 1
Reviewer 8
The uncertainties under different load scenarios provide justification for why an adaptive management approach is essential.

Page 15, Paragraph 1
Reviewer 8
This paragraph cites various sources of evidence for potential phosphorus limitation. In addition, the Rabalais et al. (1999) CENR report provides additional evidence (Table 6.1). The January and April 2004 documents also cited personal communications from various experts working in the Mississippi River outflow region. Curiously, these were omitted from the August 2004 document. A presentation at the recent American Society of Limnology and Oceanography meeting in Salt Lake City, UT further supported a prevalence of phosphorus limitation in intermediate salinity waters as demonstrated by nutrient bioassays in the river outflow region:

http://www.sgmeet.com/aslo/slc2005/viewabstract2.asp?AbstractID=750&SessionID=GS04

Page 15, Bottom Paragraph
Reviewer 8
Reference to Figure 8 should refer to Figure 9.

Page 16
Reviewer 8
Reference to Figure 9 should refer to Figure 10.

IX. ADDITIONAL REFERENCES

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